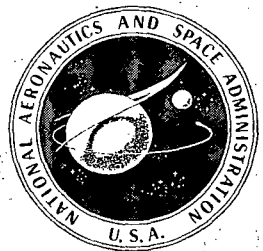


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STELLAR CHROMOSPHERES

A colloquium held at
GODDARD SPACE FLIGHT CENTER
February 21-24, 1972



NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

STELLAR CHROMOSPHERES

The proceedings of the International Astronomical Union Colloquium
held at NASA Goddard Space Flight Center
February 21-24, 1972

Edited by
S. D. Jordan and E. H. Avrett

Prepared by Goddard Space Flight Center



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PREFACE

IAU Colloquium 19 on "Stellar Chromospheres" was a natural extension of its predecessor "Spectrum Formation in Stars with Steady State Extended Atmospheres," held during April, 1969, in Munich, Germany. The present colloquium was co-sponsored by Commissions 36 and 29 of the International Astronomical Union. The official organizing committee comprised Y. Fujita, J. C. Pecker, F. Praderie, R. N. Thomas, and A. Underhill, with Underhill chairing a local, east coast organizing committee consisting of, besides herself, E. Avrett, S. Heap, S. Jordan, and D. Leckrone. The Colloquium honored Professor Cecilia Payne-Gaposchkin of the Smithsonian Astrophysical Observatory for her many outstanding contributions to astronomy. The aim of the organizers was to bring together experts on the complex radiative, hydrodynamical, and observational problems which the outer layers of stars provide, in the hope of clarifying both our present knowledge as well as where to go in the future. It is hoped that, to this end, these Proceedings will be helpful for students entering the field as well as research workers who were unable to attend.

There were no contributed papers other than the eight summary papers listed in the Contents. However, we would like to acknowledge, with our appreciation, the many participants who carefully edited their remarks and returned to us finished manuscripts complete with bibliographies, etc. We have attempted to retain the spirit and format of these manuscripts where they appear, while always being guided by the need to preserve the open, informal atmosphere of the discussions which did, in fact, prevail during the Colloquium. The final responsibility for editing is ours and, if minor changes have confused or obscured meaning, we offer the authors our apologies.

Several organizations participated in sponsoring, planning, financing, and running the Colloquium. In addition to official sponsorship by the IAU, the Goddard Space Flight Center and the Smithsonian Astrophysical Observatory were co-hosts, Goddard providing the site and direct support and the Smithsonian providing assistance in planning and a grant to defray expenses. Additional financial support was provided by a National Science Foundation Grant, and the cost of publishing the Proceedings was borne by Goddard.

Finally, it might be appropriate to point out a few salient features of the Colloquium which will certainly have bearing on future developments. The entire question of what, exactly, constitutes a chromosphere, both

conceptually, in definition, and in physical actuality, as inferred from spectral diagnostics, was discussed avidly and ardently during the sessions. The final summary and the subsequent discussion illustrate how varied are the experiences and opinions of two highly respected experts in this area. In general, the difficulties, both theoretical and observational, of studying chromospheres *in detail* still leave open many important questions which await not only improved research techniques, but improved communications between the researchers. We hope these Proceedings will serve that function for all concerned.

The Editors
Greenbelt, Sept. 18, 1972

VISITING PARTICIPANTS

- L. H. ALLER, *UCLA, Dept. of Astronomy, Los Angeles, Cal.*
 R. G. ATHAY, *High Altitude Observatory, Boulder, Col.*
 L. H. AUER, *Yale University, New Haven, Conn.*
 E. H. AVRETT, *Smithsonian Astrophysical Observatory, Cambridge, Mass.*
 J. M. BECKERS, *Sacramento Peak Observatory, Sunspot, N. Mex.*
 H. A. BEEBE, *New Mexico State, Las Cruces, N. Mex.*
 R. A. BELL, *Univ. of Maryland, College Park, Md.*
 A. M. BOESGAARD, *Univ. of Hawaii, Inst. for Astronomy, Honolulu, Hawaii*
 K. H. BÖHM, *Univ. of Washington, Astronomy Dept., Seattle, Wash.*
 E. BÖHM-VITENSE, *Univ. of Washington, Astronomy Dept., Seattle, Wash.*
 R. M. BONNET, *Laboratoire de Physique Stellaire et Planetaire, 91-Verrieres-Le-Buisson, France*
 J. P. CASSINELLI, *Joint Inst. For Laboratory Astrophysics, Boulder, Col.*
 J. I. CASTOR, *Joint Inst. For Laboratory Astrophysics, Boulder, Col.*
 R. CAYREL, *Observatoire de Meudon, 92 Meudon, France*
 P. C. CHEN, *State Univ. of New York, Stony Brook, N. Y.*
 E. G. CHIPMAN, *Laboratory For Atmospheric and Space Physics, Boulder, Col.*
 P. S. CONTI, *Joint Inst. For Laboratory Astrophysics, Boulder, Col.*
 Y. CUNY, *Observatoire de Meudon, 92 Meudon, France*
 R. J. DEFOUW, *Harvard College Observatory, Cambridge, Mass.*
 P. DELACHE, *Observatoire de Nice, Nice, France*
 L. R. DOHERTY, *Univ. of Wisconsin, Washburn Observatory, Madison, Wis.*
 B. DURNEY, *National Center For Atmospheric Research, Boulder, Col.*
 T. L. EVANS, *Royal Observatory of Edinburgh, Edinburgh, Scotland*
 R. A. E. FOSBURY, *Royal Greenwich Observatory, Greenwich, England*
 H. FRISCH, *Observatoire de Nice, Nice, France*
 C. FROESCHLE, *Observatoire de Nice, Nice, France*
 C. GAPOSHKIN, *Smithsonian Astrophysical Observatory, Cambridge, Mass.*
 K. B. GEBBIE, *Joint Inst. For Laboratory Astrophysics, Boulder, Col.*
 R. T. GIULI, *NASA Manned Spacecraft Center, Houston, Texas*
 M. GROS, *Observatoire de Meudon, 92 Meudon, France*
 M. HACK, *Trieste Observatory, Trieste, Italy*
 J. P. HARRINGTON, *Univ. of Maryland, College Park, Md.*
 S. S. HILL, *Michigan State Univ., East Lansing, Mich.*
 J. T. JEFFERIES, *Univ. of Hawaii, Inst. for Astronomy, Honolulu, Hawaii*
 M. C. JENNINGS, *Univ. of Arizona, Steward Observatory, Tucson, Ariz.*
 H. R. JOHNSON, *High Altitude Observatory, Boulder, Col.*
 W. KALKOFEN, *Smithsonian Astrophysical Observatory, Cambridge, Mass.*
 R. S. KANDEL, *Boston University, Dept. of Astronomy, Boston, Mass.*
 R. KIPPENHAHN, *Universitäts-Sternwarte Göttingen, Göttingen, West Germany*
 Y. KONDO, *NASA Manned Spacecraft Center, Houston, Texas*
 R. A. KRIKORIAN, *Inst. d'Astrophysique, Paris, France*
 L. V. KUHL, *Univ. of California, Berkeley, Cal.*
 J. W. LEIBACHER, *Joint Inst. for Laboratory Astrophysics, Boulder, Col.*
 J. R. LESH, *Joint Inst. for Laboratory Astrophysics, Boulder, Col.*
 J. LINSKY, *Joint Inst. for Laboratory Astrophysics, Boulder, Col.*
 S.-Y. LIU, *Univ. of Maryland, College Park, Md.*
 C. MAGNAN, *Institute d'Astrophysique, Paris, France*
 R. W. MILKEY, *Kitt Peak Nat. Observatory, Tucson, Arizona*

J. L. MODISETTE, *Houston Baptist College, Houston, Texas*
 H. W. MOOS, *Johns Hopkins Univ., Baltimore, Md.*
 N. D. MORRISON, *Univ. of Hawaii, Inst. for Astronomy, Honolulu, Hawaii*
 D. J. MULLAN, *The Observatory, Armagh, Northern Ireland*
 S. A. MUSMAN, *Sacramento Peak Observatory, Sunspot, N. Mex.*
 G. NESTERCZUK, *Wolf Research, College Park, Md.*
 K. NICHOLAS, *Univ. of Maryland, College Park, Md.*
 G. K. H. OERTEL, *NASA Headquarters, Washington, D. C.*
 J. M. PASACHOFF, *California Inst. of Technology, Dept. of Astronomy, Pasadena, Cal.*
 J. C. PECKER, *Inst. d'Astrophysique, Paris, France*
 D. P. PETERSON, *State Univ. of New York, Stony Brook, N. Y.*
 J. PEYTREMANN, *Harvard College Observatory, Cambridge, Mass.*
 A. I. POLAND, *High Altitude Observatory, Boulder, Col.*
 F. PRADERIE, *Inst. d'Astrophysique, Paris, France*
 N. G. ROMAN, *NASA Headquarters, Washington, D. C.*
 J. D. ROSENDAHL, *Univ. of Arizona, Steward Observatory, Tucson, Ariz.*
 G. ROTTMAN, *John Hopkins Univ., Baltimore, Md.*
 D. SACOTTE, *Laboratoire de Physique Stellaire et Planetaire, 91-Verrieres-Le-Buisson, France*
 J. SCHMID-BURGK, *University of Heidelberg, Heidelberg, West Germany*
 R. SCHWARTZ, *New York Univ., Dept. of Physics, New York, N. Y.*
 E. SEDLMAYR, *University of Heidelberg, Heidelberg, West Germany*
 N. R. SHEELEY, *Kitt Peak Nat. Observatory, Tucson, Ariz.*
 T. SIMON, *Univ. of Hawaii, Inst. for Astronomy, Honolulu, Hawaii*
 E. v. P. SMITH, *Univ. of Maryland, College Park, Md.*
 A. SKUMANICH, *High Altitude Observatory, Boulder, Col.*
 P. SOUFFRIN, *Observatoire de Nice, Nice, France*
 R. STEIN, *Brandeis University, Boston, Mass.*
 R. STEINITZ, *Joint Inst. for Laboratory Astrophysics, Boulder, Col.*
 H. H. STROKE, *New York Univ., New York, N. Y.*
 R. N. THOMAS, *Joint Inst. for Laboratory Astrophysics, Boulder, Col.*
 P. ULMSCHNEIDER, *Univ. of Wuerzburg, Astronomische Institut, Wuerzburg, West Germany*
 R. ULRICH, *UCLA, Dept. of Astronomy, Los Angeles, Cal.*
 W. UPSON, *Univ. of Maryland, College Park, Md.*
 J. C. VALTIER, *Observatoire de Nice, Nice, France*
 J. E. VERNAZZA, *Harvard College Observatory, Cambridge, Mass.*
 O. C. WILSON, *Hale Observatories, Pasadena, Cal.*
 G. L. WITHBROW, *Harvard College Observatory, Cambridge, Mass.*
 K. O. WRIGHT, *Dominion Astrophysical Observatory, Victoria, B. C., Canada*

PARTICIPANTS FROM GODDARD

**P. L. BERNACCA
J. C. BRANDT
R. D. CHAPMAN
L. DUNKELMAN
W. A. FEIBELMAN
D. FISCHER
S. R. HEAP
S. D. JORDAN**

**S. KASTNER
D. A. KLINGLESMTIH
M. LAGET
D. S. LECKRONE
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C. McCracken
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W. M. SPARKS
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A. M. WILSON
C. L. WOLFF
A. B. UNDERHILL**

PART I
SPECTROSCOPIC DIAGNOSTICS OF CHROMOSPHERES
AND THE CHROMOSPHERIC ENERGY BALANCE

Chairman: Roger Cayrel

INTRODUCTORY COMMENTS BY SESSION CHAIRMAN CAYREL

I would like to define the topic for today and then turn to John Jefferies for the first introductory paper. I understand that today's topic is twofold. First, if there is a temperature rise in a layer of optical thickness of a few hundredths in the visible, what are the features of the spectrum which are most able to detect it? That I would say is the first point. The second point is how such a temperature rise can be driven either by a radiative mechanism or by dissipation of mechanical energy.

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TEMPERATURE DISTRIBUTION IN A STELLAR ATMOSPHERE-DIAGNOSTIC BASIS

John T. Jefferies
Nancy D. Morrison
Institute for Astronomy
University of Hawaii

Presented by John T. Jefferies

INTRODUCTION

As is well known, the word "chromosphere" was coined to denote the bright, thin, colored ring seen as the solar limb was obscured by the Moon at the time of a total eclipse. This region of the Sun's atmosphere was found to be the source of many strong emission lines — the flash spectrum — some persisting to such heights as to leave no doubt that their cores originated quite high in the chromosphere. The presence of such an emission line region is not unexpected; however, what gives the solar chromosphere special interest is the fact that its observed spectroscopic properties cannot be explained on the basis that it is a simple extension of a "classical" atmosphere for which radiative, hydrostatic, and local thermodynamic equilibrium all apply. Thus, the height above the limb to which most eclipse lines persist is inconsistent with the predicted density scale height. The observation of neutral and ionized helium lines in the flash spectrum demands temperatures far in excess of those predicted for a radiative equilibrium model. Further difficulty is encountered in attempting to explain in classical terms the shapes and strengths of certain chromospheric lines in the disk spectrum, notably the self reversals in the cores of H and K. Such observations, coupled with the recognition that the coronal temperature is in the range of millions of degrees and the discovery of the peculiar inhomogeneities in the chromospheric gas, e.g., the spicules and the supergranular flow pattern and such transitory phenomena as surges, flares and prominences, all contributed to the recognition that the properties of the chromosphere are controlled by factors that lie outside the scope of a classical atmosphere. Thus, the partitioning of the Sun into photosphere, chromosphere, and corona is seen to be far more fundamental than the simple geometrical division based on eclipse observation. It appears that there are different mechanisms at work in these layers, especially in the way energy is transferred.

We recognize now that some, at least, of the spectroscopic features of the solar chromosphere are consistent with the hypothesis that the temperature increases outward above some minimum value found a few hundred

kilometers above the limb. The temperature rise is thought to be a result of the dissipation of mechanical energy generated in the photosphere, and if this is so we will naturally expect this process to take place in other stars, leading to the formation of stellar chromospheres. A direct approach to the study of these layers might be to concentrate on the kinematic motion of the line-forming layers as deduced from the shapes, strengths, and wavelength shifts of spectral lines. It is also fruitful, however, to consider the *symptom* of the dissipation of energy, namely the temperature rise, as a basis for comparison between solar and stellar chromospheres and this is the approach we shall adopt here. Thus, we shall consider a stellar chromosphere as a region where the temperature increases outward, and we shall examine spectroscopic methods for inferring the existence and properties of a temperature rise.

The following section sets out the physical basis for the discussion with some general considerations on how (or whether) the temperature structure of a gas controls the shapes of spectral lines. In particular, we shall discuss why some lines are very sensitive temperature indicators while others are much less so. Following that, we shall consider emission lines and what they can tell us about the atmosphere of the star, and we shall discuss methods for determining the temperature structure of the atmosphere from the analysis of line profiles. The final section contains a brief discussion of the information in the stellar continuum, together with some miscellaneous indicators.

THE INFLUENCE OF TEMPERATURE STRUCTURE ON LINE PROFILES

The monochromatic flux F_ν emerging from a plane parallel semi-infinite gas is given by

$$F_\nu = 2 \int_0^\infty S_\nu(\tau_\nu) E_2(\tau_\nu) d\tau_\nu, \quad (1)$$

where τ_ν is the monochromatic optical depth, E_2 is the second exponential integral, and S_ν is the source function, defined as

$$S_\nu = \frac{\epsilon_\nu}{\kappa_\nu}, \quad (2)$$

where ϵ_ν and κ_ν represent respectively the monochromatic volume emissivity and the absorption coefficient per unit length in the gas. In

general, both ϵ_ν and κ_ν will contain components from continuum and line processes; however we are here primarily interested in the cores of strong lines formed in the outer atmospheric layers, and we shall neglect the continuum contribution.

Clearly, the emergent flux will reflect the temperature distribution only to the extent that S_λ (or ϵ_λ and κ_λ) depends on the temperature. For a spectral line it is well known – see, e.g., Jefferies (1968) – that

$$S_\nu = \frac{2h\nu^3}{c^2} \left[\frac{g_2}{g_1} \frac{n_1}{n_2} - 1 \right]^{-1} \psi(\nu) , \quad (3)$$

where n_1 , n_2 are the concentrations of atoms in the lower and upper levels of the line g_1 , g_2 are the statistical weights of the levels and $\psi(\nu)$ is a function which we shall set equal to unity, following Jefferies (1968) and Hummer (1969). This latter approximation implies that the line source function is independent of frequency over the core of the line, and we shall therefore drop the subscript ν . The physical basis of our arguments remains unchanged if we neglect stimulated emission, in which case equation (3) reduces to

$$S_\ell = \frac{2h\nu^3}{c^2} \frac{g_1}{g_2} \frac{n_2}{n_1} . \quad (4)$$

Thus, the dependence of the emergent flux on the temperature structure of the gas is fixed by the temperature dependence of the population ratio. Now this ratio can be expressed, formally, as

$$\frac{n_2}{n_1} = \frac{R_{12}}{R_{21}} , \quad (5)$$

where R_{ij} is the rate of all transition paths, direct and indirect, which carry the atom from level i to level j . Recognizing that there are, in general, two mechanisms (collisional and radiative) by which transitions can take place, we can write, equivalently,

$$\frac{n_2}{n_1} = \frac{\int_0^\infty J_\nu \kappa_\nu d\nu/h\nu + C_{12} + I_{12}}{A_{21} + C_{21} + I_{21}} , \quad (6)$$

where the C 's are direct collisional rates and the first terms in numerator and denominator are respectively the direct radiative absorption and spontaneous transition rates, while the terms I_{ij} represent the rates of *indirect* transitions taking the atom from level i to level j . In this formulation, the "source" terms C_{12} and I_{12} represent the creation of fresh photons into the radiation field, while the sink terms C_{21} and I_{21} represent the destruction of absorbed photons by de-excitation of the atom. The source terms thus represent the ultimate source of the radiation in the gas.

Thomas (1957) distinguished two classes of lines according to whether direct collisional transitions or indirect processes are chiefly responsible for creation and destruction of photons. If $C_{12} \gg I_{12}$ and $C_{21} \gg I_{21}$, equation (3) reduces to

$$S_{\lambda} = \frac{\int_0^{\infty} J_{\nu} \phi_{\nu} d\nu + \epsilon B_{\nu}(T)}{1 + \epsilon}, \quad (7)$$

where $B_{\nu}(T)$ is the Planck function at the local kinetic temperature T , ϕ_{ν} is a normalized profile of the absorption coefficient and the important parameter ϵ is defined as

$$\epsilon \simeq \frac{C_{21}}{A_{21}} \quad (8)$$

Thus, ϵ measures the importance of direct collisional relative to radiative de-excitations of an atom in the upper level of the line. In this case, therefore, the gas temperature enters directly into the line source function; the physical reason is that the collisions then control the production of new photons in the line, and the rate of these collision transitions depends on the kinetic temperature, through the Boltzmann distribution. Thus, for such a "collisionally controlled" line, the atmospheric temperature structure should be reflected in the line profile. The essential questions of interest to us here are, can we know *a priori* whether a line is "collisionally controlled," and, if so, exactly how is the temperature structure reflected in the profile of the observed line?

Thomas (1957) gave a partial analysis of the first question. In particular, he showed that, for stars of solar type and later, one would expect strong resonance lines of non-metals, and of ionized metals, to be collisionally controlled. The dichotomy depends on the atomic level structure and on the color temperature of the stellar continuum; as particularly important

cases in this category, we identify the resonance lines of Ca^+ , Mg^+ , H, C, N, and O when formed in stars of solar type and later. Thomas also showed that the ratio of the populations of the levels of the resonance lines of neutral metals should be controlled less by collisions than by indirect processes, which should, in turn, be controlled by the strength of the continuum radiation field streaming through the gas. As a consequence, the source functions of such lines should *not* reflect the local temperature distribution in the region where the lines are formed, but rather the temperature in the region where the *continua* originate. Thomas' corresponding partitioning of lines into "collisional" and "photo-electric" control is important to keep in mind when designing observational programs, but it must be applied with an intelligent understanding of its basis. Thus, whether a given line falls into one or the other of the classifications depends on the gas temperature, the stellar continuum flux, and the local density; the classification is not an immutable property of the line. For example, the cooler the star, the closer a given line will be to collisional control.

Considerable insight into the question of just how sensitively the temperature structure is reflected in the line profile has been obtained over the past ten to fifteen years. For a collisionally controlled line, for which S_ν is given by equation (7), we can compute the emergent radiation for a given temperature model by solving (with appropriate boundary conditions) the transfer equation

$$\mu \frac{dI_\nu}{d\tau_\nu} = I_\nu - S_\nu = I_\nu - (1 - \lambda) \int_0^\infty J_\nu \phi_\nu d\nu - \lambda B_\nu(T), \quad (9)$$

where $\lambda = C_{21}/(A_{21} + C_{21})$ is the probability of a collisional de-excitation of an atom in the upper state of the line. We consider solutions of equation(9) for two general cases, an isothermal semi-infinite layer of gas, and secondly, a model in which the temperature increases outward.

ISOTHERMAL LAYER

Schematic results for an isothermal layer are illustrated in Figures I-1 and I-2 for a set of values of the scattering parameter λ . Two aspects of these figures should be particularly noted. Firstly, the line source function saturates to the Planck function at an optical depth of λ^{-1} as measured in the line center. This characteristic distance is known as the "thermalization length," corresponding physically to the average optical distance which a photon will travel from its point of creation as a new photon,

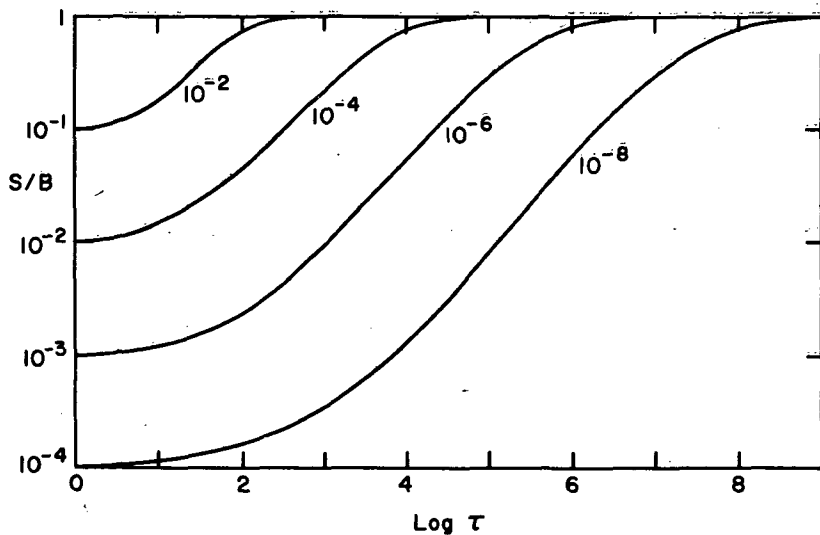


Figure 1-1 The ratio of the line source function to the Planck function, for an isothermal gas, as a function of optical depth at the line center. The different curves refer to different values of the scattering parameter λ .

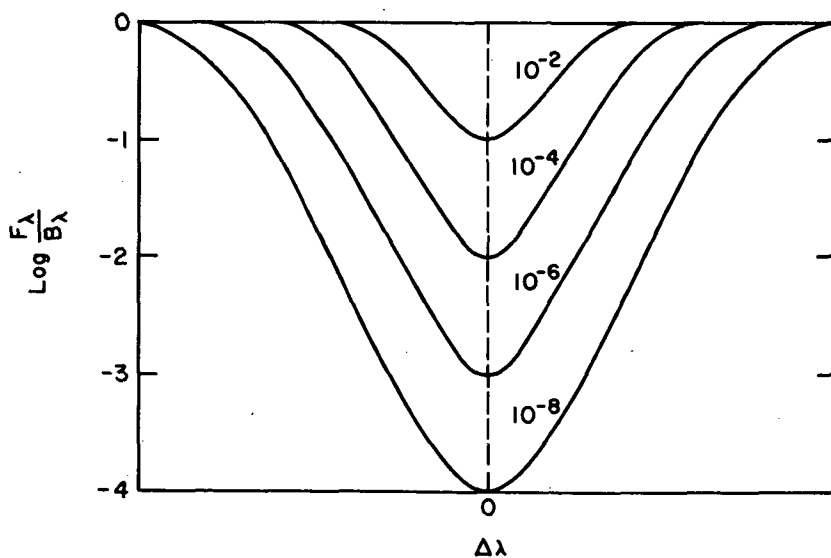


Figure 1-2 The logarithm of the ratio of the emergent flux in the line to the Planck function, for an isothermal gas, as a function of wavelength. The profiles refer to different values of the scattering parameter λ .

following collisional excitation, to its point of destruction by collisional de-excitation; this occurs, on the average, after λ^{-1} successive absorptions and reemissions. Detailed discussions of the thermalization length are given, e.g., by Finn and Jefferies (1968*a, b*) and by Hummer and Rybicki (1971*a*). It is important in the present context to point out that an equivalent interpretation of the thermalization length is the distance to which a change in atmospheric conditions will be reflected in the radiation field. Thus, for example, a discontinuous jump in temperature at some point in the gas would be reflected in the radiation field up to an optical distance (measured in the line center) of λ^{-1} away. Clearly, therefore, the degree of line scattering in the gas has a profound effect on the depth distribution of intensity in the line and so, via equation (7), on the line source function and on the profile of the emergent flux.

This fact is reflected in the second point we wish to emphasize, which is illustrated in Figure I-2. The line profiles shown there are obviously different, yet they are computed for atmospheres with *identical* temperature structure; in fact, the gas is isothermal in the kinetic temperature (although the profiles are in absorption). The differences among the profiles arise from the differences in λ (or ϵ), not from any differences in temperature structure. Only in the case of LTE (for which $\lambda = 1$) is the temperature uniquely reflected in the profile; the line is then completely filled in. From the definition (8) it can be shown that $\epsilon \sim 10^{-16} n$ for a strong line in the visible, where n is the electron density in cm^{-3} . In the solar chromosphere where Ca II H and K are formed, $n \sim 10^{11} \text{ cm}^{-3}$, consequently, $\lambda \sim \epsilon \sim 10^{-5}$, a value assuring a major departure from local thermodynamic equilibrium. Since λ is proportional to the density the profiles of collisionally controlled lines certainly reflect the gas density. In a more general case of a non-isothermal gas, we shall show that both the temperature and density distributions determine the profile; evidently, the problem of separating these two effects from the line profile will not be straightforward.

THE CHROMOSPHERIC CASE

Collisionally Controlled Lines

Jefferies and Thomas (1959) studied the formation of a collisionally controlled line in a gas whose temperature increases towards the top of the atmosphere. Their temperature model is shown in Figure I-3 as the full line; the source functions derived by solving equation (9) are shown schematically as broken curves; the corresponding emergent line profiles are shown in Figure I-4. It is immediately clear that the temperature structure is certainly reflected in the profiles but to a degree which is controlled by the

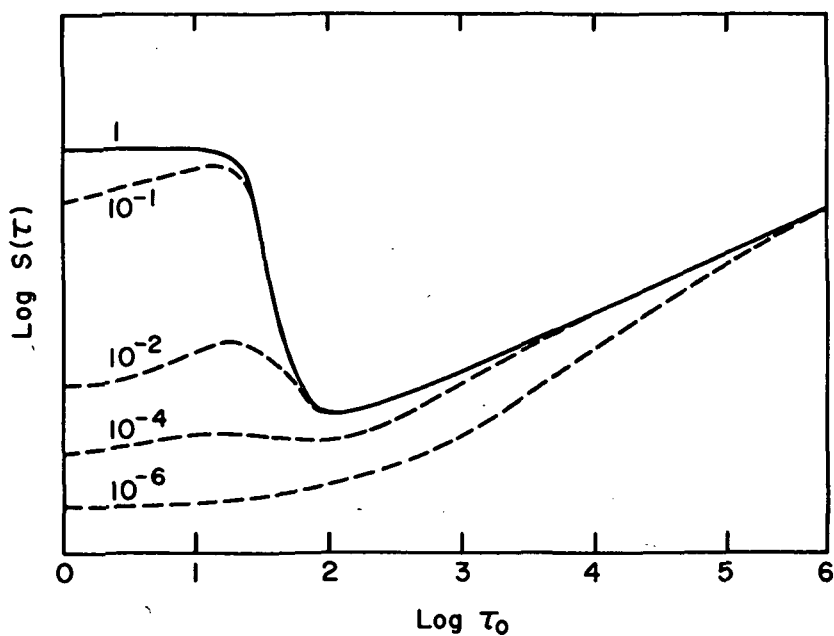


Figure I-3 The line source function as a function of optical depth in the line center. The solid curve represents the Planck function according to the model of Jefferies and Thomas (1958). The dashed curves are solutions of the equation of radiative transfer for this model, for different values of λ .

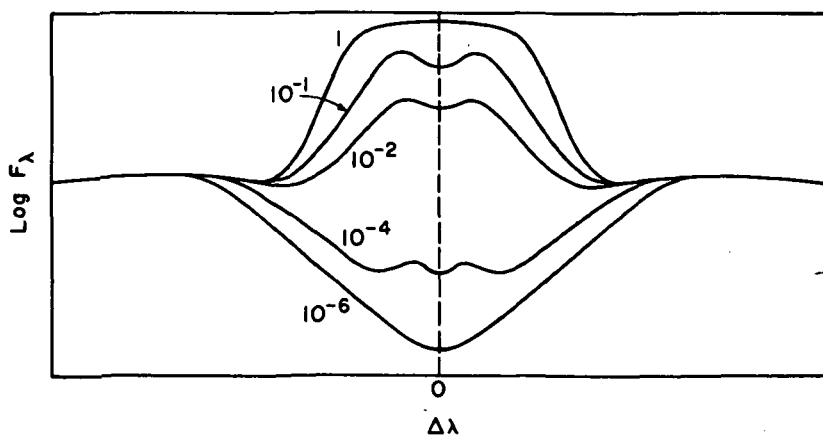


Figure I-4 Emergent line profiles predicted by the model of Jefferies and Thomas (1958) for different values of λ .

parameter λ , as would be expected from the arguments given above. There is a striking qualitative agreement between the computed line profiles and those observed in late type stars, particularly for Ca II H and K, and the Fe II (3100 Å) lines (cf. Weymann 1962, Boesgaard 1972), as well as in solar Ly α and Mg II H and K. The general consistency of the predictions is evidence that these self-reversed lines do arise in a gas with a positive temperature gradient.

The shape of the profile depends not only on the amplitude of the temperature rise, but also on the relative values of the optical depth τ_m where this rise begins and the thermalization depth $\tau^*(=\lambda^{-1})$. The K-line reversals should be or absent if $\tau_m \ll \tau^*$ and strong if $\tau_m \gg \tau^*$. Thus, emission features should go with high densities and deep chromospheres, and weak or no emissions should accompany low densities and thin chromospheres.

In summary, the observation of self-reversed emission cores in H and K give direct evidence of the existence of an outward temperature rise in the stellar atmosphere. Their absence in these lines is not, however, necessarily an indication of the absence of such a temperature rise since the density and temperature characteristics of the gas may be such that a temperature rise could not be reflected in the K line profile. It may be relevant in this regard that observations of some F stars show deep normal K line profiles and others show reversals (e.g., Warner 1968).

Photoelectrically Controlled Lines

Thomas' arguments also give us some insight into the reason why lines like H α , which are as "strong" as K, do not normally show an emission reversal. We shall not go into detail here, but merely note, with Stromgren (1935), that certain lines, of which solar H α is in fact a good example, derive their excitation mainly through indirect transitions which transfer atoms from the lower to the upper state via an intermediate state, commonly the continuum. Such processes are governed by absorption of radiation generated lower in the atmosphere, which is essentially present as a background illumination. As a result, the local temperature in a chromospheric region where the line is formed plays little or no role in determining the emergent line shape (although it may control the Doppler width and so set a scale to the profile). In that case, the line source function takes the form

$$S_{\lambda} = (1 - \eta) \int_0^{\infty} J_{\nu} \phi_{\nu} d\nu + \eta B^*, \quad (10)$$

both η and B^* being controlled by the strength and "color" of the continuous and weak line radiation streaming through the chromosphere, and so being constant with depth in line forming regions. Thus, independently of the *kinetic temperature* structure in the chromosphere, emergent profiles of photoelectrically controlled lines will have the same form as those shown in Figure I-2 since the source and sink terms for the cases illustrated there are also constant with depth. Such lines will then appear in absorption even when H and K show strong emission cores. This is not to say that H α must always be in absorption; e.g., at high densities direct collisions can become more important than indirect photoelectric processes and such a shift to collisional control is probably the reason that H α goes into emission in solar flares and in flare stars.

INFLUENCE OF INCLUDING MORE LEVELS

The simple physical arguments presented above give great insight into the response of line profiles to the temperature and density structure of a gas. For a quantitative discussion, however, greater detail is needed in the specification of the atomic level structure, particularly the incorporation of more than the two levels (plus continuum) to which earlier treatments were confined. Many calculations of multi-line problems have been carried out — cf. e.g., Avrett (1966), Finn and Jefferies (1968*b*, 1969), Cuny (1968), Athay *et al.* (1968) — but they change the above physical picture little if at all. One significant general conclusion from such calculations, however, is that the source functions of multiple lines (such as H and K) share an essentially common depth dependence over much of the gas. Waddell (1962) showed that this equality is required by a comparison of solar center-to-limb observations of D_1 and D_2 . If generally correct, the conclusion is of great importance for the analysis of stellar spectra.

SUMMARY

We have seen that profiles of certain spectral lines should be sensitive to the temperature distribution in a gas and so can presumably be considered indicators of the presence of chromospheres. We have seen that these lines are, in fact, observed to have profiles which indicate the presence of an outward temperature rise, and have seen why others, comparably strong, should not, and do not, show the same features. For the temperature-sensitive lines, we have seen that the profiles reflect both temperature and density structure, but it is not clear that we can disentangle these dependences in a unique way.

The basic physical ideas seem clear and give self-consistent (if qualitative) results. Their application to stellar problems will demand more

sophisticated computations, particularly taking into account many atomic levels in order to allow predictions on a number of lines formed in the same atom or ion.

The theoretician also faces the fact that the inhomogeneous structure seen in the solar chromosphere may be expected to be present in other stars, and he must seek to compute its influence on the space averaged profiles observed from a star. The averaged spectrum from a multidimensional medium does not necessarily reflect average temperature or density conditions, but the extent of this failure is not yet clear. The techniques for studying such questions are available in Monte Carlo programs or in the more conventional solution of three-dimensional transfer problems, and it seems that only by model calculations can we obtain some idea of the sensitivity of different lines in stellar spectra to inhomogeneities. Such data are essential if we are ever to develop sound methods of analysis, or even to design meaningful observation programs.

DO EMISSION LINES IMPLY A CHROMOSPHERE?

The problem of what the presence of an emission line implies about the structure of a stellar atmosphere is still unsolved. Following Gebbie and Thomas (1968), we can characterize the problem in the following specific terms: The observed flux is given as

$$F_{\nu} = \int I_{\nu} \mu \, d\omega \quad , \quad (11)$$

and the central question is whether the emission line is intrinsic, over all or some of the star's surface (i.e., $I_{\nu} > I_c$), or whether it has a geometrical origin because the area of integration is much greater for the line than for the continuum. It is of basic importance to try to develop a diagnostic tool to discriminate between these two possible sources of emission lines. Although we have no concrete ideas to suggest here, a reasonable first step would be to study some model problems to clarify the consequences of postulating a geometrical origin for emission lines. The theoretical tools for handling such problems are available, especially since the development (cf. Hummer and Rybicki, 1971 b) of simpler methods for handling transfer problems in spherical atmospheres. A model problem based on a pure hydrogen atmosphere could greatly clarify this question.

A line will be *intrinsically* in emission if the line and continuum source functions are related according to the inequality,

$$S_Q(\tau_0 = 1) > S_c(\tau_c = 1) , \quad (12)$$

where the optical depths are measured along the direction of observation, and τ_0 refers to the line center, τ_c to the continuum at a neighboring wavelength. We may obviously satisfy this inequality either by reducing S_c or by increasing S_Q . The latter possibility occurs most naturally, at least for a collisionally controlled line, if the temperature increases outwards. This mechanism explains the fact that, in the solar atmosphere, emission lines are abundant below ~ 1600 Å (and present up to ~ 2000 Å). To some extent, their appearance is favored by the increasing continuum opacity below about 1800 Å which places the region of formation of the continuum near the temperature minimum, while that of the lines lies in the chromosphere.

The alternative notion that S_c is depressed below S_Q was originally explored by Schuster (1905), later by Underhill (1949) and more recently by Gebbie and Thomas (1968). In its simplest form, and the one most favorable for emission line formation in a "classical" atmosphere, Schuster's mechanism supposes that the line is formed in LTE so that $S_Q = B_\nu$, while the continuum is formed partly by thermal and partly by scattering processes so that S_c is given as

$$S_c = (1 - \lambda_\nu) J_\nu + \lambda_\nu B_\nu(T) , \quad (13)$$

with $\lambda_\nu < 1$.

For an *isothermal* gas, $S_c/B_\nu(T)$ will be less than unity near the front of the atmosphere because the escape of photons from the surface reduces J_ν below B_ν . Consequently, inequality (12) is satisfied and the line appears in emission. For a normal radiative equilibrium gradient, however, the continuum intensity J_ν increases substantially and it becomes much more difficult to satisfy the inequality (12). Gebbie and Thomas (1968) concluded that, except perhaps in the infrared, the Schuster mechanism would be ineffective in a classical atmosphere. Their conclusion, supported by the work of Harrington (1970), is only strengthened if the line source function is represented by the more physically correct expression (7). In this case, the emergent central intensity drops below its LTE value, making it still more difficult for the line to appear in emission. The applicability of the Schuster mechanism is further reduced by the fact that it requires that the continuum not be formed in LTE; for most stars, however, LTE is believed to hold in the continuum. Still, exceptions exist, especially for hot stars, where electron scattering is significant

while, even for the Sun, LTE fails below the Lyman limit. Hence, the possibility that S_c is reduced by some departure from LTE in the continuous spectrum needs to be kept in mind in connection with the appearance of emission lines in a stellar spectrum.

We believe that a rich field of investigation of great potential to stellar spectroscopic diagnostics is to be found in a concentrated attack on the appearance of emission lines in stars. So far, the confusion between processes forming intrinsic emission lines and those arising from extended envelopes (stationary or expanding) has limited our ability to use these lines for diagnostic purposes. The sophistication of present-day computational methods is sufficient, and the rewards sufficiently attractive, to merit a full-scale attack on the problem of differentiating between these two entirely different origins for emission lines.

ANALYSIS OF SPECTRAL LINES

OPTICALLY THICK GASES

We saw above that certain lines are expected to contain information in their profiles on, among other things, the distribution of temperature with depth in the gas. We now wish to discuss briefly the problem of using the information in an observed line profile to infer the temperature distribution in the gas; in a sense, this is the inverse of the problem, discussed above, of computing the line profile given the atmospheric structure.

The best starting point currently available is the expression

$$F_\nu = 2 \int_0^\infty S_\nu(t_\nu) E_2(t_\nu) dt_\nu, \quad (14)$$

which already restricts the scope of our analysis to a homogeneous semi-infinite plane — parallel layer. A more complicated expression suitable for spherically symmetric geometry could no doubt be obtained; an extension to more general expressions incorporating stochastic spatial variations is beyond the present development of the subject.

From equation (14), the first part of the analysis consists in determining from the observed profile F_ν the run of $S_\nu(t_\nu)$ for each point on the line profile. As it stands, however, infinitely many possible distributions $S_\nu(t_\nu)$ will satisfy the integral equation (14). Some limitation of these solutions can be obtained if we restrict attention to those parts of the profile where continuum processes are negligible compared to those in the line, so that S_ν

and t_ν refer only to the spectral line. However, even in that case, the depth distribution of S_ν is not uniquely determined. In order to invert equation (14) uniquely, Jefferies and White (1967) have shown that it is necessary to have observational profiles of two or more lines whose source functions at all depths are related in some known way. Since, as mentioned above, studies of multiline transfer problems have indicated that the source functions of close-lying multiplet lines are essentially equal at all depths, except perhaps close to the surface, such lines should provide the necessary data for an analysis. This principle has been applied by Curtis and Jefferies (1967) to the solar D lines (for which the availability of center-to-limb data greatly simplifies the problem, and allows us to retrieve information on the continuum parameters also). Wilson and Worrall (1969) have also attempted an analysis of the solar D lines, using data at one point on the disk; their procedure is essentially equivalent, therefore, to that which would in practice be applied to stellar spectra, where no geometrical resolution is obtainable. It is not our purpose here to discuss in detail such analytical methods, or the closely related method of deJager and Neven (1967), but rather to draw attention to their existence, since they offer an alternative interpretive method to that based purely on model calculations. The theory of such analytical processes also allows a more incisive study of such important questions as the uniqueness of a particular derived model, a subject quite beyond the scope of this paper but one nevertheless deserving closer study than it has received.

While the analytical method has promise, at least, of determining the depth variation of S_ν and the frequency and depth variation of the line absorption coefficient, its application so far (to solar data) has not been wholly satisfying. The difficulty may lie in inadequate data, in uncertainties in the inversion of the integral equation, in limitations in the basic formulation (14), or in the degree to which S_ν is independent of wavelength within the line and the same from one line to another.

If such problems can be resolved, the depth variation of S_ν would still require interpretation in terms of the density and temperature structure of the gas. We can see no way of approaching this other than through a model calculation. At least, the depth and wavelength dependence of the line absorption coefficient that is yielded by the analysis would be helpful by setting constraints on the model.

OPTICALLY THIN GASES

The specific intensity emitted from a gas in an optically thin line reflects the integral of the volume emissivity along the line of sight; in general, the line profile does not reflect the way in which emitting material is

distributed and an infinite number of geometrical rearrangements of the emitting material will yield the same emission in all optically thin lines. A satisfying technique for spectroscopic diagnosis of an optically thin line would therefore seek some way of specifying the physical state of the gas which is unique and so preserved under such geometrical rearrangement. This general problem has been studied by Jefferies, Orrall, and Zirker (1972). While their particular interest lay in its use for the analysis of coronal forbidden lines, the method is of general application, in particular to the optically thin lines of the solar chromosphere.

For a transparent gas, the specific intensity I , integrated over the line profile, can be written

$$I = \frac{h\nu}{4\pi} A \int_0^\infty n_u(x) dx, \quad (15)$$

where n_u is the population of the upper state of the line and x is the geometrical coordinate in the line of sight. The emissivity is controlled only by the local electron density and temperature, and the intensity of interacting radiation fields of known strength, provided that n_u is determined by electron collisions or by the absorption of radiation in spectral regions which are themselves thin. In the usual way, we expand the population n_u in the form

$$n_u = \frac{n_u}{n_i} \frac{n_i}{n_A} \frac{n_A}{n_H} \frac{n_H}{n} n, \quad (16)$$

with n_i the concentration of the ionization stage to which the line belongs, n_A and n_H the concentrations of the element and of hydrogen, and n the electron density. The ratios n_i/n_A , n_u/n_i , and n_H/n are all functions of n and T only. If we define

$$a_{el} = \frac{n_A}{n_H} \quad (17)$$

as the abundance of the element with respect to hydrogen and define an ionization-excitation function

$$\chi(n, T) \equiv \frac{n_u}{n_i} \frac{n_i}{n_A} \frac{n_H}{n}, \quad (18)$$

then equation (15) takes the form

$$I = \frac{h\nu}{4\pi} A a_{el} \int_0^{\infty} \chi(n, T) n \, dx. \quad (19)$$

Specific distributions $n(x)$ and $T(x)$ would, of course, characterize the gas uniquely and would yield straightforwardly a value of I . However, as stated above, we could never determine such distributions uniquely from the observed intensities. We therefore abandon the geometrical distribution as a characterization of the gas and instead transfer the analytical problem to an n, T space by introducing a distribution function $\mu(n, T)$ through the definition

$$dN(n, T) \equiv N \mu(n, T) \, dn \, dT, \quad (20)$$

with $dN(n, T)$ the number of electrons in the sampled column that are in neighborhoods where the electron temperature lies between T and $T + dT$ and, simultaneously, the electron density lies between n and $n + dn$. The distribution dN is normalized to the total electron content N in the column so that $\mu(n, T)$ is normalized to unity. In these terms we can write

$$I = C a_{el} N \int_0^{\infty} \int_0^{\infty} \chi(n, T) \mu(n, T) \, dn \, dT, \quad (21)$$

where $C \equiv (h\nu/4\pi) A$. Equation (21) is a double integral equation with kernel $\chi(n, T)$ which may, in principle, be solved for the distribution function $\mu(n, T)$, given data on the number of lines for which the functions χ are sufficiently different.

While, in principle, we might hope to infer the bivariate function $\mu(n, T)$, in practice we shall probably have to accept the more limited description of the gas implicit in the assumption that n and T are uniquely related everywhere along the line of sight.

In that more restrictive case, equation (21) becomes

$$I = C a_{el} N \int_0^{\infty} \chi[n(T), T] \phi(T) \, dT, \quad (22)$$

where the distribution function $\phi(T)$ is given by

$$\phi(T) = \int_0^{\infty} \mu(n, T) \, dn \quad (23)$$

and $n(T)$ is the single valued function relating the electron density to the temperature. Clearly $\phi(T) \, dT$ is the fraction of all the electrons in the column whose temperatures lie between T and $T + dT$.

The finesse of an analysis based on the above formulation will depend on the degree to which the excitation-ionization functions $\chi(n, T)$ differ from one line to another. Since we can calculate χ in advance once we know the cross sections for radiative and collisional transitions, we can decide in advance which set of lines of a particular ion will best suit our needs for analysis.

ZETA AURIGAE-TYPE ECLIPSING BINARIES

Because of their special geometry, a class of eclipsing binaries present favorable cases for the study of a stellar chromosphere. Of the bright stars of this type, the prototype ζ Aur is the best observed but the observational results are similar for 31 and 32 Cygni — cf. Wilson (1961), Wright (1970). These binary systems are composed of a K-type supergiant and an early B-type dwarf or subgiant which undergoes total eclipse. As it passes behind the extended atmosphere of the supergiant, absorption lines appear in the spectrum. Since the radiation field of the B star may affect the temperature structure of the chromosphere of the K giant it is not clear to what extent results from these systems apply to single stars. In the absence of any other way of obtaining direct detailed information about the temperature structure of a star other than the sun, the method nevertheless has great value.

In spite of this fact, few observers have attempted to draw conclusions about the variation of temperature with height in the chromosphere. Those who have done so have used a curve-of-growth analysis for the line spectrum to derive values for the excitation and ionization temperatures at one or several heights in the chromosphere. In their study of ζ Aurigae, Wilson and Abt (1954) were able to reproduce their observations only by supposing the envelope to be slumpy. Otherwise, the B-type star ought to ionize the envelope of the supergiant to a greater degree than that observed. Wright (1959) concurred that the chromospheric spectrum ought to be produced mainly in small condensations where the density is much

greater than in the rest of the gas. Further evidence for the existence of condensations is given by the observation that the chromospheric K line usually contains several components of different radial velocities. As in the Sun, then, a correct analysis must take into account the inhomogeneity of the medium.

MISCELLANEOUS INDICATORS

SYMBIOTIC FEATURES

Unambiguous indications of the presence of a temperature rise are given by what we will call symbiotic spectral features: features whose behavior in a stellar spectrum yields, through elementary analysis, values of temperature or abundance that are anomalous or disagree with the values derived from other spectral features in the same star. For example, calculation of the population of the lower level for conditions expected in stars yields an estimate for the strength of the line at a given temperature. If the line has a large excitation potential and is stronger than expected, it must arise in a hot layer above the photosphere. For example, the Balmer lines in some M-type giants are anomalously strong, indicating overpopulation of the second level by a large factor (Deutsch 1970). For a Orionis, Spitzer (1939) showed that the great strength of H α implies a radiation density of Ly α that corresponds to a temperature of 17000°K. This value contrasts sharply with the effective temperature of the star, which is near 3300°K. Another example of this type of indicator is a group of lines near 1 micron wavelength due to Si I and Mg I (Spinrad and Wing 1969). Since they have excitation potentials of about 6 eV, their presence is favored by temperatures of 5000° or 6000°K. Nevertheless, they are as strong in α Ori as they are in the Sun. Finally, the most important symbiotic features are the lines arising from excited states of He I. Though λ 10830 is the most prominent of these lines, others, such as λ 5876, may be observable in cool stars also. Vaughn and Zirin (1968) calculated the population of the lower level of the line at 10830 Å and found that, for all densities considered, it is negligible for $T < 20000^\circ\text{K}$ and large for all higher temperatures. This line must therefore be regarded as a clear indicator of a large rise in temperature in the upper atmosphere of any star whose effective temperature is substantially less than this value.

CONTINUOUS SPECTRA

For a region of a stellar continuous spectrum where the opacity is known as a function of wavelength, the wavelength variation of the emergent

flux will contain information about the temperature gradient. In some regions of the spectrum, the opacity may be so high that even the continuous radiation arises effectively in the chromosphere; in the sun this happens for millimetre radiowaves, and again in the near UV at about 700 Å. If the color temperature, or the brightness temperature, of the radiation increases as the opacity increases, an outward temperature rise is indicated.

An example of such a case is found in the ultraviolet below about 0.3 μm , where a high opacity is provided by the bound-free continua of hydrogen and the metals. The opacity generally increases toward shorter wavelengths, and, at some wavelength, the continuous radiation originates effectively at the height in the atmosphere where the temperature has a minimum. Naturally, this wavelength is shorter than the wavelength at which the chromosphere begins to influence the cores of the lines and where, consequently, emission lines begin to appear. Since the transition of the line spectrum from absorption to emission occurs at longer and longer wavelengths for later and later spectral types, it is reasonable that the influence of the chromosphere on the continuum should also extend to longer wavelengths for later spectral types. Doherty (1970) has studied the ultraviolet continua of K and M stars near 3000 Å as observed by OAO-2. In particular, he considered the wavelength dependence of the color temperature of the continuum, which should reflect the variation of the electron temperature with height. For stars of spectral type earlier than about K5, the flux below 0.28 μm decreases rapidly toward shorter wavelengths. For α Tauri and α Orionis, however the flux decreases much more slowly in this region, and both stars show a minimum in the color temperature at about 0.30 μm . Whether this minimum indicates a temperature minimum in the stellar atmosphere or only a maximum in the opacity is not clear.

Another source of continuous opacity that may be important to this discussion is the H-ion. Beyond 1.6 μm , the free-free opacity of this ion increases monotonically, in a known manner (Geltman, 1965). If, in the chromosphere of a cool star, the temperature is low enough and the electron density is high enough, the H-ions might produce enough opacity so that the continuous radiation in the observable infrared would arise in the chromosphere. Thus, limb brightening or even an infrared excess might be observable at wavelengths shorter than 20 μm . Noyes, Gingerich, and Goldberg (1966) searched unsuccessfully for limb brightening at 24 μm in the Sun. From a model of the solar chromosphere, they predicted that the Sun should show an infrared excess at 50 μm . They suggested further that other stars, in which there is an additional opacity source in the infrared or in which the temperature minimum lies at greater optical

depth than in the Sun, might show an infrared excess at shorter wavelengths.

Another source of excess radiation at long wavelengths might be free-free emission from hydrogen. If the characteristics of the long-wave radiation were to require that the source have an electron density lower than that expected in the photosphere, the radiation would have to arise in an extended envelope surrounding the star. If, in addition, the temperature required for the source is substantially higher than the effective temperature of the star, a temperature rise above the photosphere is indicated. For example, Wallerstein (1971) has considered whether the excess at $10\ \mu\text{m}$ of the KO supergiant W Cephei could be produced by free-free emission. He found that the size (but not the wavelength dependence) of the infrared excess could be produced by free-free emission in a sphere with radius $15\ \text{A.U.}$ if the electron density is $2 - 4 \times 10^9\ \text{cm}^{-3}$ and the electron temperature is $5000\text{--}6000^\circ\text{K}$. Since the effective temperature of a KO supergiant is only about 4000°K (Allen 1963, p. 201), the star presumably has a chromosphere, but the free-free emission, which comes from a very extended region, apparently does not originate there.

In the Sun, free-free emission at radio frequencies arises in the corona. From a simple model of a stellar corona, Weymann and Chapman (1965) have predicted that free-free emission should be detectable in the microwave region, and this emission has been detected in cool stars (Kellermann and Pauliny-Toth 1966; Seaquist 1967).

CONCLUSION

In this review, we have tried to indicate areas where further theoretical work would improve the present understanding of stellar chromospheres. In several places, we have emphasized that calculations of line profiles need to take into account inhomogeneities in the gas. This necessity arises from the fact that all stars that can be observed with spatial resolution over the disk — the Sun and the ζ Aur variables — show inhomogeneities in the chromosphere. Past work on the ζ Aur variables has shown that the effect of inhomogeneities can be striking.

We have also considered the problem of obtaining the distribution of temperature with height from an observed line profile. Although we pointed out that such methods exist and should be applied to stellar spectra, we also noted that the methods cannot yet be applied with complete success. Not only are better line profiles needed than are usually obtainable from stars, but further improvements in the theory are also required, from improvements in the basic formulation to refinements in numerical techniques.

The most general area of research that we have suggested is, however, the question of what emission lines mean. We mentioned three situations that are thought capable of producing emission lines in a stellar atmosphere: a temperature rise, the Schuster mechanism, and the case where the effective emitting area is larger in the line than in the continuum. Of these suggestions, only the first is thought to exist generally; still, the others cannot be entirely ruled out. It would be desirable to know in detail what conditions would permit the Schuster mechanism or the geometrical mechanism to operate. This area of research promises to be one of the most fruitful in the area of stellar chromospheres.

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DISCUSSION FOLLOWING THE INTRODUCTORY TALK BY JEFFERIES

Skumanich — There is a hidden parameter in Jefferies' curves which I think should be brought out, namely, the thickness of the chromosphere or, conversely, the scale height. If the optical thickness of the chromosphere is held constant and there are changes in the density, then the physical thickness must change. Such a change is governed by the momentum equation (e.g. hydrostatic equilibrium).

Jefferies — You are quite right. I should have mentioned that for illustration I took a constant optical thickness for the chromosphere and varied λ independently.

Skumanich — This makes three parameters not two, and we should worry about the variations of all three.

Jefferies — Yes, that's correct, and the relationship of the thickness of the chromosphere to the density is of fundamental importance. In point of fact, if the optical depth where the chromospheric temperature rise begins

is greater than the thermalization length then you'll have strong chromospheric emission, and if it's less the emission will be weak.

Athay — There is another aspect of the uncertainty coming into the profile, and that is the opacity of the atmosphere. Even the photoelectrically controlled lines depend on temperature due to the temperature dependence of the line opacities.

Skumanich — One more parameter is that which describes the kinematic situation, and this brings the number of basic parameters to four.

Poland — In reference to the statement that an increasing source function is the result of a chromosphere, it should be mentioned that Auer and Mihalas have obtained results that show it is possible to get emission lines due to optical pumping without a chromosphere. For example HeII lines can be pumped by hydrogen lines if the overlap is sufficient.

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STELLAR-CHROMOSPHERIC MODELS

Eugene H. Avrett

*Smithsonian Institution
Astrophysical Observatory*

ABSTRACT

In "Types of Theoretical Models" we describe two basic types of theoretical models — radiative equilibrium and empirical — that are used to represent stellar chromospheres. The next Section is a summary of recent work on the construction of radiative-equilibrium model atmospheres that show an outward temperature increase in the surface layers. Also, we discuss the chromospheric cooling due to spectral lines. In "Solar Empirical Models" we describe the empirical determination of solar-type chromospheric models that, in order to match observations, imply a temperature rise substantially greater than that predicted by radiative equilibrium. Such a temperature rise must be largely due to mechanical heating. An attempt is made in the concluding Section to apply a scaled solar chromospheric model to a star with a different surface gravity. The results suggest that the chromospheric optical thickness is sensitive to gravity and that the width of chromospheric line emission increases with stellar luminosity, in qualitative agreement with the width-luminosity relationship observed by Wilson and Bappu.

TYPES OF THEORETICAL MODELS

Current research on chromospheric models can be described in terms of two different approaches. The first involves calculating a theoretical spectrum on the basis of a set of *a priori* assumptions that includes the assumption of radiative equilibrium. Often a grid of such models is constructed for different values of effective temperature, surface gravity, and composition. The calculation of radiative-equilibrium models has reached a new level of sophistication recently with the detailed inclusion of non-LTE effects (Auer and Mihalas, 1972) and line blanketing (Kurucz, Peytremann, and Avrett, 1972).

These models may account for most of the observed features in normal stellar spectra, but they do not account for the chromospheric spectra of late-type stars such as the Sun. As discussed in the following section, the

only point of controversy in the solar case is whether or not radiative equilibrium plays even a minor role in the initial chromospheric temperature rise.

The second approach is the same as the first except that in place of the radiative-equilibrium assumption, which fixes the temperature distribution, we adjust the temperature versus depth by trial and error until the computed spectrum agrees with the observed one. An empirical model for the solar chromosphere is obtained in this way, as discussed below. The solar spectrum has been observed throughout different wavelength regions in such detail that we can test our theoretical models for consistency: Typically, we have a greater number of spectral features to match than parameters to adjust.

Once a detailed empirical chromospheric model is obtained for the Sun, or for any well-observed star, it is possible to calculate the mechanical energy flux as a function of depth, i.e., the amount that must be added to the radiative flux to make the total flux constant with depth. A knowledge of the mechanical flux distribution should lead us to an understanding of the nonradiative heating mechanism, and then perhaps to a method by which this flux distribution can be calculated for any star. As a result, we would be able to construct realistic chromospheric models based on the assumption of radiative equilibrium with mechanical heating. Despite the work still to be done, this goal seems within reach.

RADIATIVE-EQUILIBRIUM MODELS

Here we summarize recent work on the construction of model atmospheres in radiative equilibrium that show an outward temperature increase in the surface layers.

Auer and Mihalas (1969a, b, 1970) have calculated non-LTE radiative-equilibrium model atmospheres for hot stars with effective temperatures of 12500° and 15000°K . They examine the heating of the outer layers caused by a positive flux derivative in various continuum wavelength intervals when J_ν exceeds the continuum source function. They find that the main source of heating is due to photoionization in the Balmer continuum and that this is mostly a population effect: The H α line provides an efficient channel for 3 to 2 transitions causing a greater level 2 population, greater heating, and a surface temperature rise. The line itself tends to cool the atmosphere, but by an amount smaller than the heating caused by the change of level populations.

Feautrier (1968) also computed non-LTE model atmospheres in radiative and hydrostatic equilibrium with effective temperatures 15000° and

25000°K and $\log g = 4$, and with a solar effective temperature together with both $\log g = 2$ and solar gravity. He includes departures from LTE in H^- as well as in hydrogen. In the higher effective-temperature atmospheres, he finds surface-temperature increases of as much as 1000° or 2000°K in agreement with Auer and Mihalas, but in the solar effective-temperature cases, he finds increases of only a few hundred degrees.

The physical mechanism responsible for the surface-temperature rise was pointed out by Cayrel (1963); it is an extension of the classical radiative-equilibrium model of a planetary nebula, as discussed, for example, by Baker, Menzel, and Aller (1938). Essentially, there is a shift from LTE in the underlying star to unbalanced radiative equilibrium in the low-density outer atmosphere, where the temperature is close to the color temperature of the star, rather than to the lower classical boundary temperature.

Skumanich (1970) has recently discussed the validity of the Cayrel mechanism in response to an earlier suggestion by Jordan (1969) that radiative equilibrium is incompatible with a departure of the continuum source function from the Planck function for atmospheres of large H^- concentration.

Gebbie and Thomas (1970, 1971) discuss the role that collisions play in the energy balance. They find that the low chromospheric densities are too high to be neglected in calculating the temperature and atomic populations. Hence, the planetary-nebula type of calculation does not give correct results for the low chromosphere. They discuss the determination of the temperature distribution in terms of transfer effects and population effects and, as a measure of population effects, introduce a quantity they call the "temperature control bracket," defined as the photoionization rate divided by the corresponding integral containing the monochromatic source function instead of J_ν .

Most of the above studies are concerned with heating due to photoionization. A number of other recent studies have been made of radiative-energy losses and of cooling due mainly to lines.

Dubov (1965) emphasizes that the main factor responsible for the cooling of the chromosphere is radiation in separate spectral lines. Athay (1966) discusses the energy loss from the middle chromosphere due to the hydrogen Balmer lines. Frisch (1966) estimates the cooling due to collisional excitation in various lines and finds that, in the vicinity of the temperature minimum, the energy losses due to the Ca and Mg H and K resonance lines together with the Ca infrared triplet are approximately half those due to radiative recombination. Athay and Skumanich (1969)

and Athay (1970) carry out extensive non-LTE line-blanketing calculations and find that the tendency for the temperature to rise in the surface layers due to the Cayrel mechanism is strongly resisted by the effects of line blanketing; they also find that a chromospheric rise of 300° or more would require a substantial input of mechanical energy.

Hence, it is by no means certain that even the initial temperature rise in the low solar chromosphere occurs as a consequence of radiative equilibrium.

SOLAR EMPIRICAL MODELS

In this section we discuss the empirical determination of model chromospheres, such as that of the Sun, for which the temperature rise is substantially greater than that predicted by radiative-equilibrium calculations. The results shown here are from the study of Linsky and Avrett (1970) of the solar H and K lines. The model has been chosen such that the predicted microwave spectrum lies within observed limits and the computed H- and K-line profiles resemble those observed from quiet regions near the center of the solar disk. This model is intended to be only a representative one. Empirical solar models that also match various features in the extreme ultraviolet have been constructed more recently by Noyes and Kalkofen (1970), Gingerich, Noyes, Kalkofen, and Cuny (1971), and Vernazza, Avrett, and Loeser (1972).

Disk-center brightness temperatures observed in the region 10μ to 2 cm are shown in Figure I-5. The solar continuous opacity increases with increasing wavelength in this region, so that radiation at longer wavelengths is emitted by layers at greater heights in the atmosphere. The spectrum shortward of about 300μ originates in the photosphere, and that longward, in the chromosphere.

The solid line in Figure I-5 represents the brightness temperatures we computed based on the temperature-height distribution shown in Figure I-6. The abrupt temperature increase that begins at about 7500° has been introduced to account for the Lyman-continuum spectrum shortward of 912 \AA and to keep the computed gas pressure above coronal values (see Athay, 1969, Noyes and Kalkofen, 1970).

Unfortunately, there is a second function of height that must be introduced in order to specify the model. In our study of the calcium lines, we need to introduce non-thermal Doppler broadening to explain the observed central line widths. The line absorption coefficient at the wavelength λ for an atom of mass M has the Doppler width

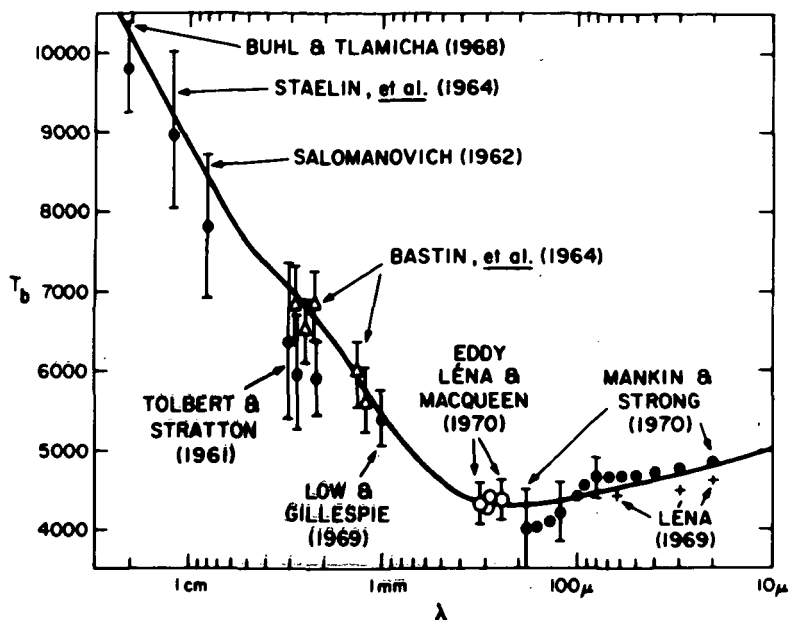


Figure I-5 Comparison of the observed and calculated brightness temperatures of the disk center. References to the papers indicated in the figure are given by Linsky and Avrett (1970).

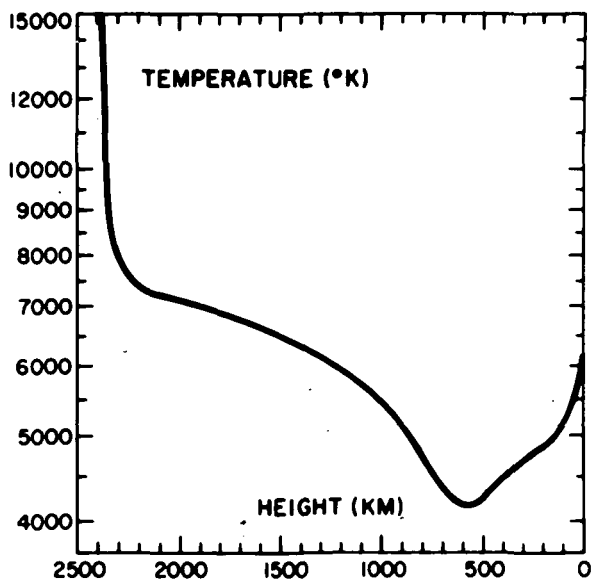


Figure I-6 The assumed temperature-height distribution.

$$\Delta\lambda_D = \frac{\lambda}{c} \sqrt{\frac{2kT}{M} + V^2} ,$$

where T is the temperature at the given depth. We use the parameter V as a measure of any required nonthermal Doppler broadening. Central profiles of the Ca II infrared triplet lines, formed between 500 and 1000 km, indicate values of V in the range 2 to 3 km/sec. Higher in the atmosphere, where the H- and K-line centers are formed, V must exceed 4 km/sec. We have attempted to adjust $V(h)$ to obtain good agreement between the calculated and the observed line profiles. The result is shown in Figure I-7.

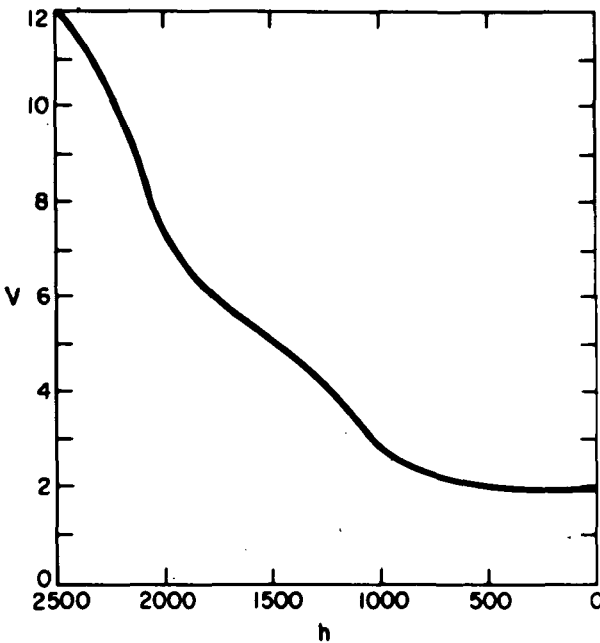


Figure I-7 The nonthermal velocity distribution used in line broadening and in the pressure equation.

We have chosen to use $V(h)$ also to represent a nonthermal contribution to the total pressure P . We let

$$P = P_g + \frac{1}{2} \rho V^2 ,$$

where P_g is the gas pressure and ρ is the density. This added pressure term extends the model in height and gives better agreement with observed eclipse scale heights.

Given $T(h)$ and $V(h)$, we solve the equations of hydrostatic equilibrium, statistical equilibrium, and radiative transfer for atomic hydrogen, taking into account the ionization and excitation of other constituents as required. Figure I-8 shows the resulting ground-state hydrogen number density n_1 , the electron and proton densities n_e and n_p , the electron pressure P_e , the total pressure P , and the turbulent pressure $P_t = 1/2\rho V^2$.

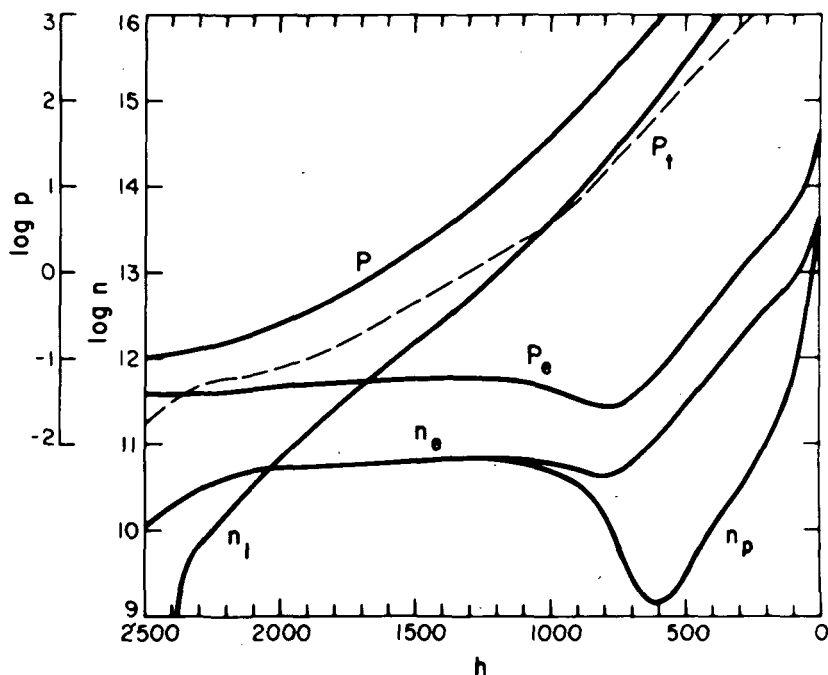


Figure I-8 Distributions of pressure and number density, including n_e the electron density, n_p the proton density, n_1 the density of hydrogen atoms in the ground state, P_e the electron pressure, P_t the turbulent pressure, and P the total pressure.

Having established the atmospheric model, we solve the transfer and statistical-equilibrium equations for Ca II. Figure I-9 shows the computed frequency-independent source function for the K line plotted against height and against line-center optical depth. This source function is a measure of the ratio of upper and lower level number densities. If this

ratio were given by the Boltzmann equation, as in LTE, S would be equal to the Planck function B , which is also shown for comparison.

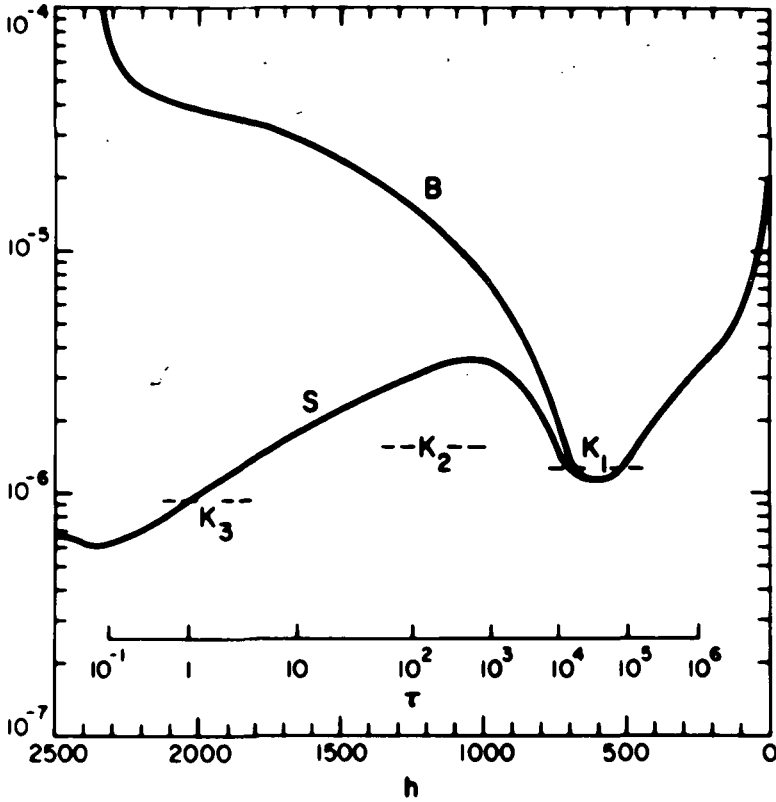


Figure I-9 The K-line source function, Planck function, and line-center optical depth. The computed line-intensity values at K_1 , K_2 , and K_3 are indicated by dashed lines.

The computed H- and K-line intensity profiles for the center of the solar disk are shown in Figure I-10, compared with those observed by White and Suemoto (1968). We plot the average of the red and violet halves of each observed profile. Residual intensities are plotted in Figure I-10, but the absolute intensities of K_1 , K_2 , and K_3 (the minimum, peak, and central values) are shown for reference in Figure 5. The K_2 peak intensity is substantially less than the maximum of S because of the Doppler-width variation with height in this region. The agreement between calculated and observed profiles shown in Figure 6 is the best we obtained after many trial adjustments of $T(h)$ and $V(h)$.

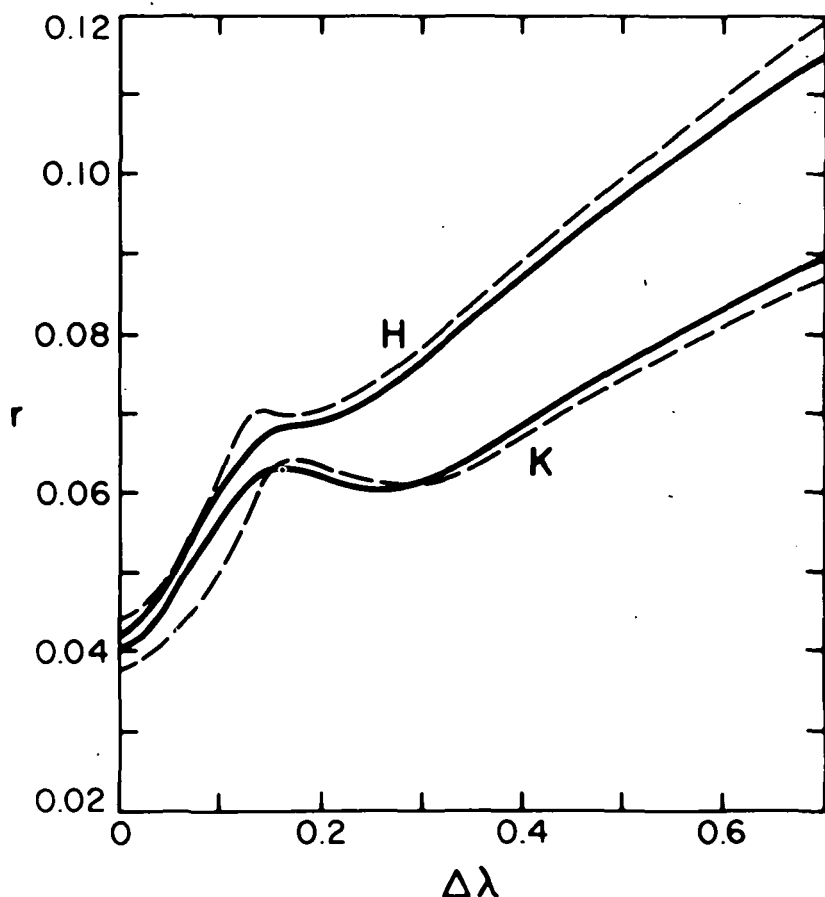


Figure I-10 The computed disk-center H- and K-line profiles (broken lines) compared with the corresponding observed profiles of White and Suemoto (1968), (solid lines).

These emergent line profiles are calculated assuming that the monochromatic line source function S_ν is equal to S throughout the line. This assumption is valid in the line core, where Doppler redistribution takes place, and in the far wings, where $S = S_\nu = B$. In the intermediate wings, several Doppler widths from line center, the situation is unclear. In this region the coherent-scattering approximation $S_\nu = J_\nu$ may be more accurate; if so, the computed line profile may have a different shape between K_2 and K_1 .

The quiet-Sun K_2 emission peaks are weak and subject to various fluctuations from point to point on the disk. It may be that we should

try to match not the spatially averaged profiles shown in Figure I-10, but the ones observed with high spatial resolution.

It is of interest to note the difference in shape between the quiet-Sun H and K profiles shown in Figure I-11 and the plage profiles in Figure I-12. The question of whether S_{ν} is closer to S or J_{ν} in the intermediate line wings might be answered by a theoretical study of plage profiles. The published research on coherence and noncoherence in the K-line wings is discussed in Section III.2 of the review by Linsky and Avrett (1970).

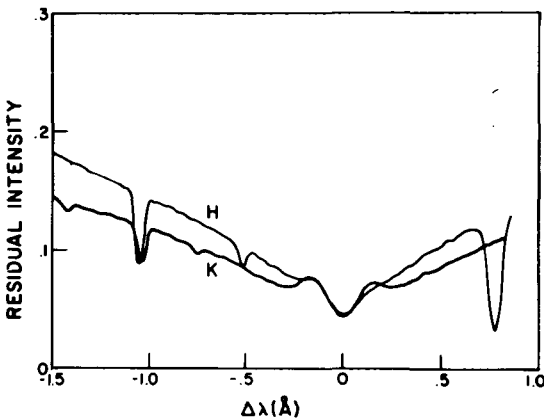


Figure I-11 Low-spatial-resolution residual intensities of the H and K lines for quiet regions near the disk center, as obtained by Linsky (1970). Although the K line exhibits a distinct double reversal, the H line exhibits only a plateau in the violet wing and no reversal at all in the red wing.

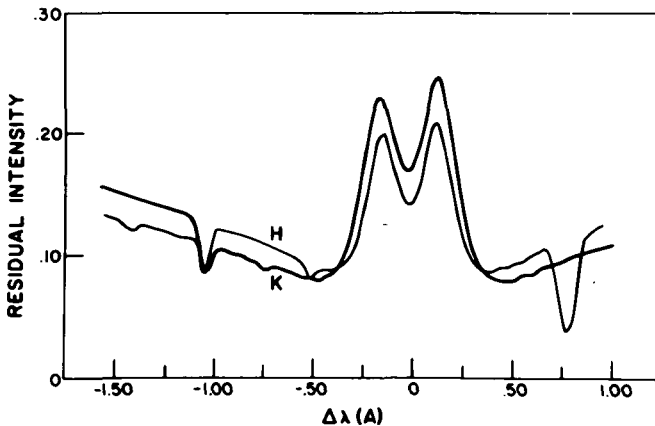


Figure I-12 Low-spatial-resolution residual intensities of the H and K lines for a plage region, as obtained by Linsky (1970).

THE EFFECT OF GRAVITY ON CHROMOSPHERIC THICKNESS

In this final section we attempt to apply a scaled solar chromospheric model to a star having a different surface gravity. Figure I-13 shows the solar temperature distribution to be used for this purpose. This $T(h)$ differs somewhat from the earlier one shown in Figure I-6 because we have made changes in the corresponding $V(h)$. The photospheric temperature distribution from zero height ($\tau_{5000} = 1$) to the temperature minimum is approximately in radiative equilibrium. The right-hand portion of Figure I-14 shows the calculated photospheric temperature distribution for a star with a solar effective temperature but with $\log g = 2$. The $\log g = 2$ photosphere is more extended in height by a factor of about 250, which is approximately the ratio of the two values of g . We have arbitrarily chosen a chromosphere for this star that is scaled from the solar model by roughly the same height factor. Note that the calculated chromospheric τ_{5000} scale is very different in the two cases. The computed number densities are shown in Figure I-15: Those for $\log g = 2$ are about a factor of 10 smaller than the corresponding solar values. However, the $\log g = 2$ scale height exceeds that of the Sun by the much larger factor 250. Whenever the opacity is proportional to n_H , as it is for the K line, we expect the $\log g = 2$ chromosphere to have a greater optical thickness.

The K-line source function and line-center optical depth for the two cases are shown in Figure I-16. In this figure and in the preceding one, the $\log g = 2$ height scale appears at the top and the solar height scale at the bottom. Note that at the temperature minimum, τ_K for $\log g = 2$ is an order of magnitude greater than $\tau_K(\text{solar})$. This increased thickness leads to a greater width of that portion of the line that originates above the temperature minimum. When the thickness is greater, we need to look farther out in the line wings to see the photosphere. Figure I-17 shows the two computed flux profiles.

These results illustrate a plausible effect of a change in gravity: The lower-gravity atmosphere is less dense but geometrically extended to a greater degree. The outer layers then have greater optical thickness, which leads to a greater line-emission width and the geometrically extended atmosphere tends to have a greater luminosity. Hence, the width W increases with luminosity L . The degree to which these results appear consistent with the observed relationship $W \propto L^{1/6}$ found by Wilson and Bappu (1957) will be discussed later at this meeting by Dr. E. Peytremann.

Further attention should be given to the shape of the computed profiles shown in Figure I-17. The observed stellar profiles appear to have a

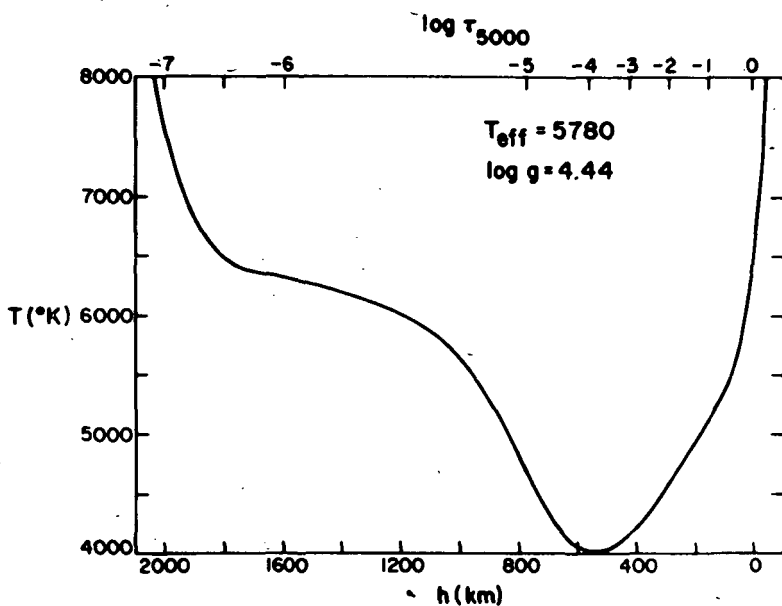


Figure I-13 The solar, $\log g = 4.44$, temperature distribution used for comparison with another case for which $\log g = 2$.

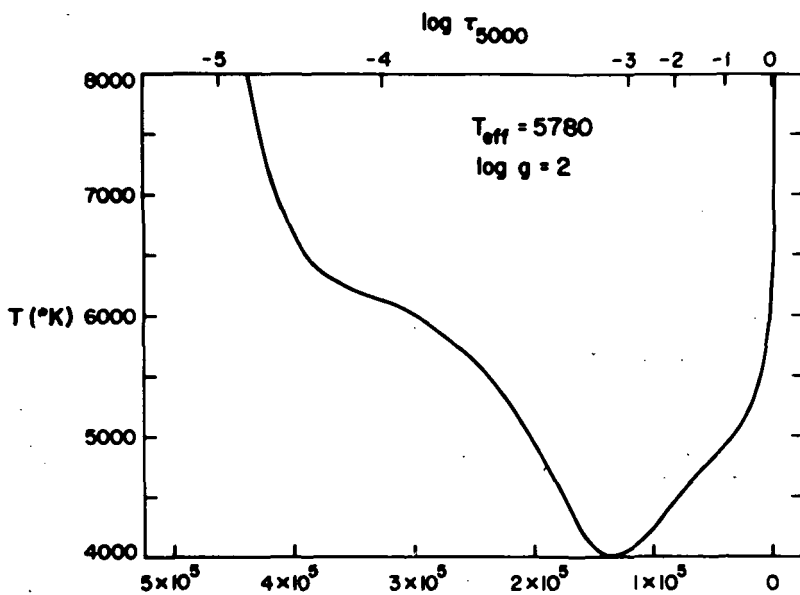


Figure I-14 The adopted $\log g = 2$ temperature distribution.

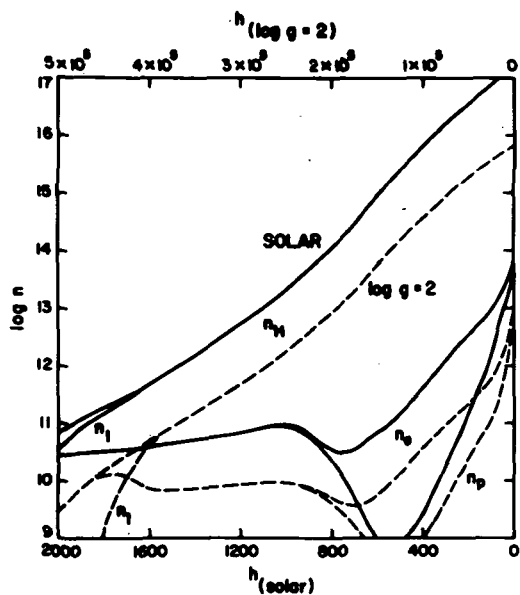


Figure 15 A comparison of the number densities in the two cases.

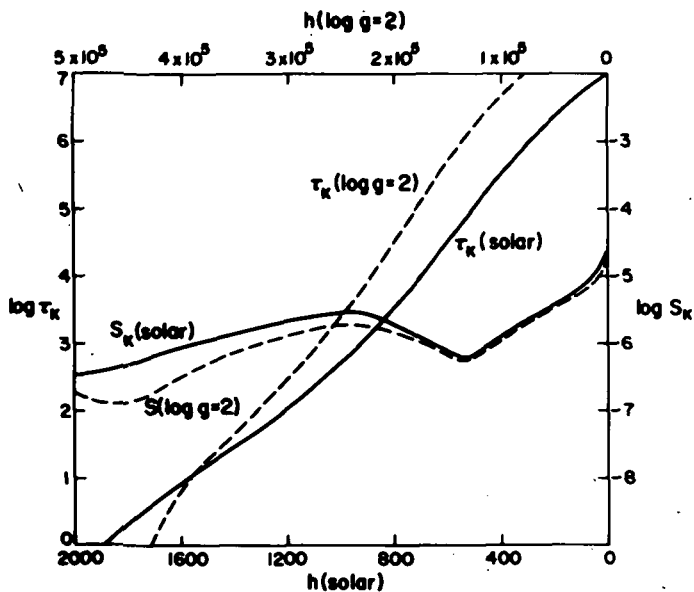


Figure 16 The K line source function and line-center optical depth in the two cases.

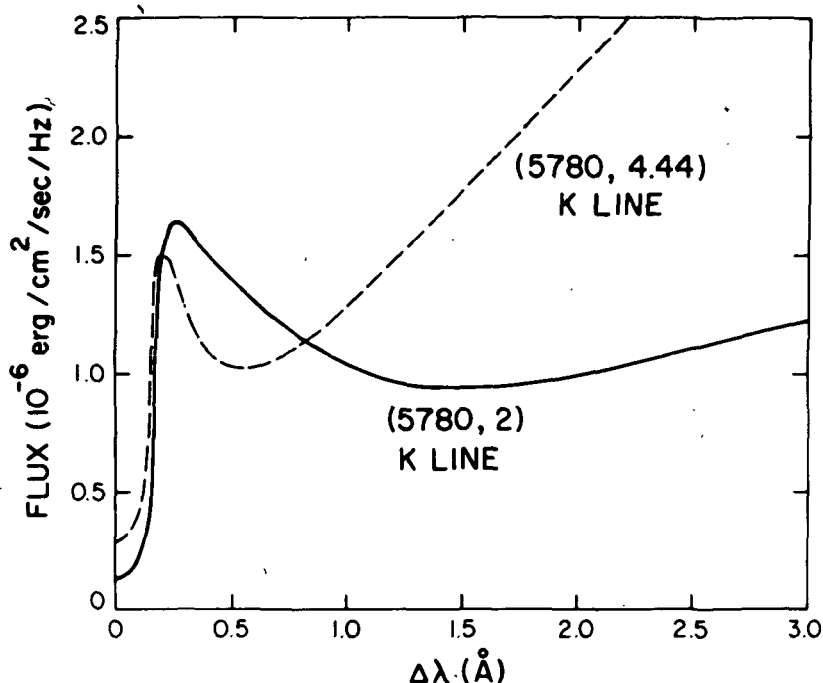


Figure I-17 The computed flux profile for the K line in the two cases.

sharper transition between the K_2 emission peak and the K_1 minimum (see, for example, Griffin, 1968, and Liller, 1968). Perhaps the transition between Doppler core and damping wings should occur farther out in the line. Also, as noted in the last section, we should examine the effects of *partial coherence in the region between K_2 and K_1* .

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DISCUSSION FOLLOWING THE INTRODUCTORY TALK BY AVRETT

Aller — I should like to ask about the suggested theoretical one-sixth power relationship between calcium emission width and visual luminosity.

Avrett — We find an increased width with decreasing gravity, which in turn is normally associated with an increased luminosity. In the session tomorrow Eric Peytremann will show the results we have to date and how they compare with the Wilson-Bappu relation. To summarize them, the $\log g = 2$ case with a temperature similar to that of the sun turns out with a reasonable mass determination to fit the Wilson-Bappu relation within the error bars. The only other calculation we have done so far is for an effective temperature of 6000° with $\log g = 4$; the error bars again include the Wilson-Bappu relationship but they are very large. At the moment these results are only schematic. Also our choice of a chromosphere in the non-solar cases was completely arbitrary. We have to see whether we just happened to select chromospheres which give the proper optical thickness for the calcium emission.

Jefferies — This is more of a comment than a question. One of the things which bedevils comparison between observation and theory of model atmospheres is, of course, the question of the uniqueness of any derived model. In order to characterize an atmosphere fully one needs to introduce a substantial number of parameters. Because of this, one needs

to compare computed profiles for *many* lines and obtain good agreement with observation for all of them before one can have confidence in the model. Hence, while an observation of the K line is valuable, its value is greatly enhanced if it is accompanied by simultaneous observations of the other lines of ionized calcium.

Linsky — I should like to second what Jefferies has said concerning the need to observe many lines together. I have found from bitter experience that observations of the calcium K line contain insufficient information to define a unique model for a chromosphere or even a chromospheric structure in the Sun. One can always trade off temperatures against densities or broadening parameters at one height, or trade off properties at one height against those at another and obtain the same computed line profile. To surmount this problem we have obtained data, as we will show this afternoon, on the infrared triplet lines as well as the H and K lines of Ca II in a number of solar plages. These lines differ in opacity by about a factor of 200. One surprising thing that we have found is that it is not always true that chromospheric emission appears in the more opaque lines before it appears in the weaker lines. We find that the 8498 Å line, the weakest of the infrared triplet lines of Ca II, shows emission before the more opaque 8542 Å and 8662 Å lines of the triplet.

Underhill — The discussion up to now has necessarily concentrated on solar type objects. These objects can be used as an anchor to confirm the theory, and the development of theory is partly based on explaining solar observations. However, other stars have chromospheres. I should like to ask the theoreticians if there has been any attempt to examine how the theory of the classification of lines into classes which are collision-dominated or into photo-electrically dominated classes must change as the temperature and the total radiation field changes. The density of stellar atmospheres changes from cooler to hotter stars — the atmosphere of a main-sequence B star is essentially the same as that of a G giant — so it seems to me to be possible that a collision dominated line will change to a radiation dominated line as the peak density of the radiation field changes its wavelength range and the density of the atmosphere is reduced. Has anyone any views on this question?

Thomas — The rules for that were set up when the original classification scheme was presented. You can calculate the collision rate and the radiation rate or any of the other indirect rates. Recently there is the work of Auer and Mihalas in which the Ca lines and the Mg lines become photoionization dominated.

Auer — I would like to comment on some of the work by Mihalas and myself on the atmospheres of very hot stars and to clarify the point

raised by Poland. The primary feature of the non-LTE atmospheres, which we constructed, is a temperature rise at the surface caused by radiative heating. The models predict that the Paschen α line of hydrogen (and presumably the higher α transitions also) is an emission line. This effect is caused by a combination of the temperature rise and the fact that the infrared lines are formed high in the atmosphere. In the region where these lines are formed the collision rates are low and the dominant way out of a state is a cascade to a lower state. The temperature rise aggravates the rate of recombination and, therefore, the rate of emission. The situation is somewhat similar to the planetary nebula case.

This mechanism does not suffice to produce emission in the $\lambda 4686$ line of HeII, which is observed to be in emission in Of stars. We attempted to produce this emission by using the Bowen mechanism. The $2n$ to $2n'$ transitions of HeII overlap the n to n' transitions of H, and therefore one might expect pumping of the $2n'$ levels of HeII. If the upper level of a transition is strongly overpopulated, an emission line will result. Such is the theory, but unfortunately the results are not in good agreement with the observations. There is a tendency for emission, but not nearly as much as the observations require and $\lambda 10124$ is predicted to be in absorption while it is observed to be in emission in ζ Pup.

Cayrel — Is there any observation supporting the calculations of emission in Paschen α ?

Auer — Yes. There's another thing I should have mentioned. Helium 5876 and 6678 are also predicted to be weakly in emission at very high temperatures. It would appear that if you are looking for evidence of a temperature rise at the surface of an O star, you should look at the strong lines in the infrared.

Praderie — Would you produce emission in H alpha also and could you say how it would vary as you change the gravity?

Auer — The calculations that we have indicate that at the very highest temperatures the cores are beginning to go into emission, just very slightly. If you had an eclipsing binary and you observed it just at eclipse then you should see it strongly in emission. Unfortunately such binaries are few and far between and often have structures complicated with circumstellar gas. Normally H alpha remains in absorption over the entire range. But to make definitive statements about a strong line like H alpha one really should know more about motions in the upper layers of the stellar atmosphere.

Skumanich — I want to raise a word of caution about the broadening velocities Avrett used. There is observational evidence that velocities are

larger in the giants than in the main sequence stars, in which case the use of the scale of the main sequence amplitudes is incorrect. I'm wondering whether, in fact, using constant energy relations for this broadening, like $pv^2 = \text{const}$, to go from main sequence stars to the giant stars may not be a better approximation. Then you might indeed find that you're not on the damping portion of the absorption coefficient curve but still in the Doppler part and you're not getting this kind of variation then. So it's not the actual thickness of the chromosphere that's changing with g but perhaps the broadening that is still changing.

Cayrel — That's a very fundamental point. Can Dr. Olin Wilson perhaps comment on that?

Wilson — I have always liked the velocity broadening but there is nothing sacred about that assumption. I'll wait until all the returns are in.

Peterson — Along that line there is observational evidence that exists for turbulence following the mv^2 relationship.

Pasachoff — May I remind the assemblage that for the Sun we have another way we can look at the surface of a star besides the methods used to produce the very lovely results we heard discussed this morning. We can look at the chromosphere sideways at the limb. Many people here, particularly the HAO, Sac Peak and Hawaii groups, have eclipse results that show the intensities of many lines at the limb very accurately. There are lines of many elements besides calcium. Even outside eclipse we can also study the oxygen infrared triplet, the D_3 line of helium, and with a little more difficulty the 10830 line of helium. There are thousands of rare earth lines. I recently made observations at the Sacramento Peak Observatory of the ionized titanium lines near 3760 Å, and the resonance line of ionized strontium at 4077 Å. Jacques Beckers, also at Sacramento Peak, has observed a whole sequence of ultraviolet chromospheric lines at the limb, which I am now studying. One can study outside of eclipse much more than the relative intensities of the various lines, which all appear in emission. However, there are calibration and scattered light difficulties, and one can't study the height structure as well as at an eclipse. We have spoken of models of turbulent velocities varying with height; we should study the velocity variation with height by actually following the spectral lines out from the edge of the sun.

Thomas — Could I just make a point of basic principle here. Sometime during this meeting maybe one should have a popularity vote on whether you want the chromosphere to begin where the temperature rises, or where you put the mechanical heating in which guarantees the rise above what you would have from a purely photospheric radiative equilibrium

model. It's a point one must carefully distinguish, particularly in view of Auer's remarks on the basic characteristics of chromospheres in hot O and B stars referring to his and Mihalas' calculations of models with no mechanical heating. I have my own position which is that the mechanical energy input fixes the chromosphere. But that's something everybody has to decide for himself. So maybe we should think about it.

Cayrel — Yes. In fact I have noted that nobody has really cared very much about the definition of the chromosphere which was involved in the topic of this morning.

Kandel — I don't want to define a chromosphere, but I think we ought to be more specific about the temperature. We all understand that when we talk about the temperature structure we are talking about the electron temperature, and in some way the energy content of the electron gas. This is not the temperature of the atmosphere as a whole. When we get the source function of H and K, we have a measure of the excitation temperature of the CaII gas, and when we talk about energetics we also have in mind some sort of temperature, but of the gas as a whole. We're interested in energetics which perhaps depend on electron temperature which, in principle, we get from continuum measurements and hydrogen ionization and excitation but which, in practice, we seem to have a hard time getting. What we want to do is find out how to determine these things, namely, the specific energy content of the gas in terms of the observables, the calcium populations, and other things. So perhaps we should keep in mind what we mean by a temperature.

Thomas — Electron temperature is always the thing which one has in mind in all these kinds of calculations. I couldn't agree more with your premise and I'd like to know what partition of energy one has over all the energy levels. But so far we have again taken the theological position that there is an electron temperature that defines the velocity distribution of electrons, and that defines all collisional parameters. That's the only reason for doing it.

Kandel — I think when we talk about a given temperature structure we're talking about an *electron* temperature structure. If we talk about an isothermal atmosphere this doesn't mean that the specific energy content does not vary through the atmosphere, and there is really no reason to be surprised at finding absorption lines coming from such an atmosphere.

Pecker: I want to make a simple reply to the question of what is a chromosphere. Jefferies spoke at the beginning and said that the symptoms of a chromosphere are an increase of the electron temperature. This is a much too closed definition. You might have heating without having

heating by mechanical energy, or you might have heating without an increase in temperature outward. I do not think that an outward temperature increase alone determines whether or not there is a chromosphere.

Cayrel — I am not sure I have understood your point. If one star with no dissipation of mechanical energy at all has an outward rise in temperature, and if a second star has some dissipation, but not large enough to cause a temperature reversal would you say that the second star has a chromosphere but the first one does not?

Pecker — Yes.

Underhill — I think that the definition of a chromosphere should not consider the question of temperature, however defined. The chromosphere is that outer part of a stellar atmosphere where you have to consider the physical processes in detail.

Thomas — I would really like to comment on this point. Suppose we divide the star into two parts: interior and exterior. Then our aim is to try to make general structural models of stars from the standpoint that the atmosphere is the transition region between the stellar interior and the interstellar medium. As a whole, a star is a non-equilibrium, non-steady-state object. Basically it is a storage pot of energy and mass. The interior is characterized by the fact that the primary focus is on population of energy levels and concentrations of mass particles and you can compute all of these without caring at all about what the fluxes are, using standard LTE distribution functions. You compute the distribution of those TE parameters specifying the distribution functions by always using a diffusion approximation. This is a linear non-equilibrium thermodynamic equilibrium situation. Then consider the other part of the star, the atmosphere. There we are mainly concerned with propagation phenomena. We want to characterize the whole sweep of the atmosphere as a gradual unfolding from a completely degenerate aspect in the interior, which is locally in thermodynamic equilibrium in the broadest sense, to the interstellar medium, an almost completely non-degenerate configuration, not in LTE in any sense. Then we divide the atmosphere into a number of subregions. We characterize each subregion by the unfolding of some aspect of this kind of degeneracy which represents the general thermodynamic equilibrium state. The reason I introduce this now was in answer to Anne Underhill's comment. It is not just the chromosphere where we begin to worry about the detailed physical characteristics, it is already in the photosphere. What is the basic point? In the sub-atmospheric regions we have a storage of electromagnetic energy and a storage of mass because we have a kind of diffusion approximation

characterizing the transfer of process in either case. The photosphere is characterized by an increasing direct escape of photons from the star. So we have in this region the gradual beginning of all those aspects of non-LTE which affect populations of energy levels associated with the fact that the photons can escape directly from the boundary and there is no longer, to a first approximation, an isotropic radiation field. The chromosphere we characterize as that region where we begin to have a departure from the storage properties of the mass flux. Go back to Eddington's old approximation in his representation of a Cepheid. He had a standing wave as far as the mass transfer and the kinetic energy transfer in the stars were concerned. Where did the model begin to lose energy? Only in the non-adiabatic part where one has a radiation field. The evolution from this thinking applied to a Cepheid atmosphere came in Schwarzschild's work where running waves were introduced in the upper part of the atmosphere. This is analogous to that thing which produces the chromosphere now — forget the details about convection, turbulence, etc. — producing acoustic waves that run out. In the Cepheid we have a system of standing waves in the interior. Suppose, for example, we had a zero minimum temperature at the top of the photosphere (which is the easiest way to look at it), then we'd have all the energy in trapped waves which leak a bit of energy at their top. This leakage is provided by "diffusion" through the system of standing waves in the subatmosphere. It's exactly in analogy with the storage of all the electromagnetic energy in the sub-atmosphere, with leakage from the diffusion approximation, balancing the surface loss, due to direct escape of photons at the boundary in the photosphere. So the photosphere is that part of the atmosphere which represents for electromagnetic energy, a transition from sheer storage with a little bit of leak in the sub-atmosphere to direct escape from the photosphere. The chromosphere is that region where I have a macroscopic escape of energy in the mechanical degrees of freedom, that is, progressive waves going out, as contrasted to the storage properties which hold at the bottom of the chromosphere. So I have then two distinct atmospheric regions: the photosphere and the chromosphere. We think we can do the same kind of thing in the corona in terms of direct mass loss from the star. I would just like you to focus on the physics here: in the photosphere it's the photons, in the chromosphere it's the mechanical energy, in the corona it's the mass. All this should come after what Francoise Praderie is talking about tomorrow; she demonstrates it much more clearly than this. That's why I would buy the chromosphere as the place where we have a mechanical dissipation of energy, because a photospheric temperature rise, as Cayrel and Helene Frisch have very carefully pointed out, has nothing to do with anything except photons and the way in which they are linked to the interaction

with matter; namely, inelastic collisions are negligible, and we simply have photoionization for the opacity processes considered.

Underhill — I want to make sure that we understand your definition of the chromosphere as the place where mechanical energy is dissipated. Also, we have to consider the end of this conference at the same time as the beginning. You say you are going to talk about a star where we have mass loss. I would like to say that many early type stars are known to have mass loss from direct observations. I don't think we can have mass loss following your types of arguments, which are physically logical to me, without also saying you have a chromosphere. Therefore, I'm going to say quite happily that I can talk about chromospheres for stars of type A, B and O. Is that logic irrefutable?

Thomas — I'll buy chromospheres for all types of stars.

Linsky — This morning the subject came up of the Ca II H and K lines as indicators of stellar chromospheres. I think that it is relevant, therefore, to present some observations that Richard Shine and I at JILA have obtained in the calcium lines for solar plages and a sunspot. This work will be the basis of his thesis. At present we have reduced the observations and are now in the process of building model chromospheres to explain the data. The data are all photoelectric and were obtained in a double pass at Kitt Peak.

In Figure I-18 we call your attention to what the calcium lines look like in the average quiet solar chromosphere. Incidentally, if the Sun were

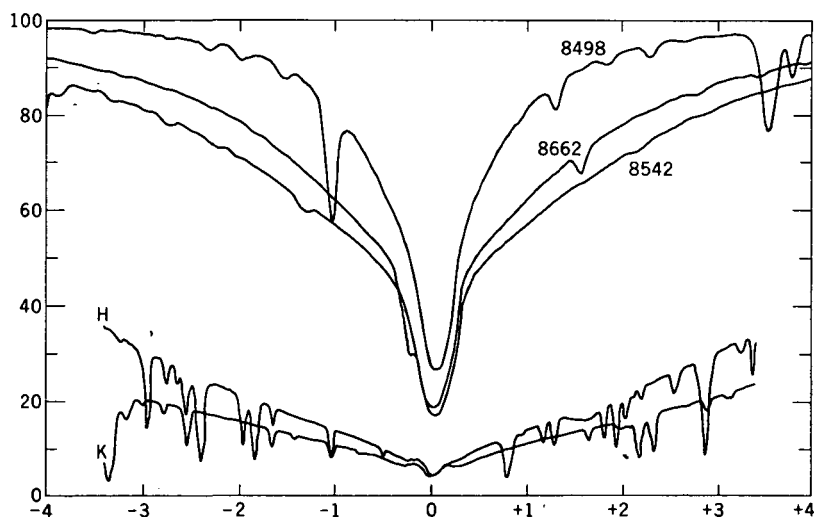


Figure I-18

observed as a point source, the profiles would be essentially the same as are seen in the quiet chromosphere. In this and subsequent figures we show the H and K resonance lines as well as the infrared triplet lines (8498, 8542, and 8662 Å). As you recall the ratio of gf values and thus of opacity are 1:5:9 for the 8498, 8662, and 8542 Å lines respectively. It is important to remember that the 8498 Å line, is by far the weakest in the triplet. In these figures we give residual intensities for the lines relative to the interpolated continua at line center as a function of wavelength measured from line center. In the quiet chromosphere the infrared triplet lines show no emission and H and K exhibit weak emission. Also the residual intensities in the cores of H and K are about the same.

Figure I-19 shows the five calcium lines in the weakest plage we observed. As has been known for some time, the cores of H and K show emission

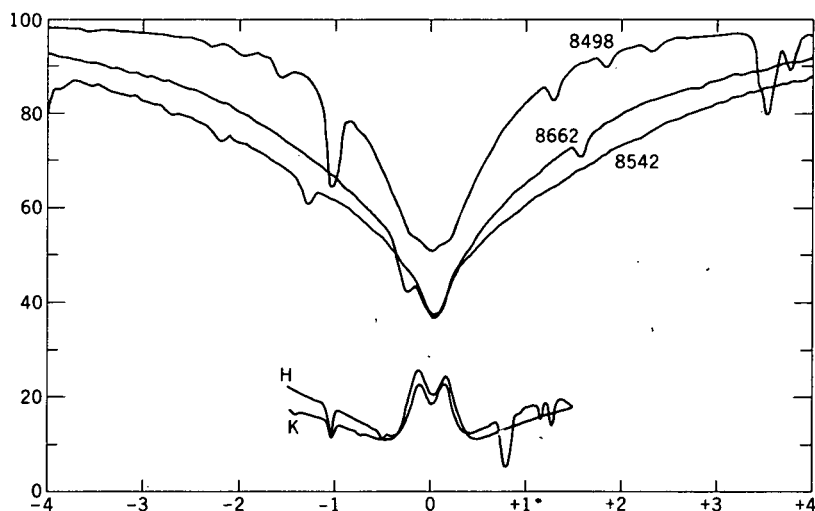


Figure I-19

and also broaden appreciably. K shows more emission than H with the ratio of residual intensities about 1.1 instead of 1.0. This ratio persists for all plages we observed. Also the residual intensities in the cores of the infrared triplet lines have increased significantly relative to the quiet chromosphere. What was unexpected in a weak plage was that the 8498 Å line, the least opaque of the infrared triplet lines, shows a definite double reversal in its core. In a slightly stronger plage, seen in Figure I-20, there

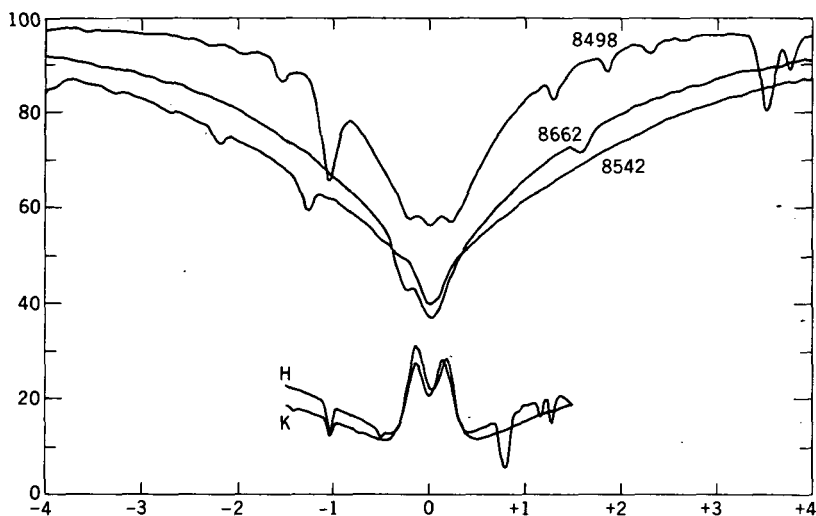


Figure I-20

is also a definite double reversal in 8498 but not in the other infrared triplet lines. This phenomenon is thus real and may place an important constraint upon acceptable models for weak solar plages. It also says that the 8498 Å line may be a very sensitive indicator of stellar chromospheres of stars similar to the Sun.

In the strongest plage we have observed, all five calcium lines, as shown in Figure I-21, show emission features and K_2V is 42% of the continuum. The double reversal in the 8662 Å line is exaggerated by an iron line just to the violet of line center. Note that the 8498 Å line shows a narrower and stronger emission feature than the other two infrared triplet lines.

In a sunspot, shown in Figure I-22, an entirely different set of profiles appear. The infrared triplet lines show no emission whereas the resonance lines show narrow emission features in their cores. The emission feature in K is much brighter than that in H with the ratio about 1.6. I suspect that an explanation for the calcium line profiles in a sunspot will require a much thinner chromosphere as measured in K line center optical depth units and a much steeper temperature gradient for the chromosphere of a spot relative to a plage.

Finally I would like to show an unexpected phenomenon in the wings of the calcium lines. In Figure I-23 we show the calcium lines for the strongest and weakest plages and for the quiet chromosphere. Note that the wings of the lines for the strong and weak plages are identical and

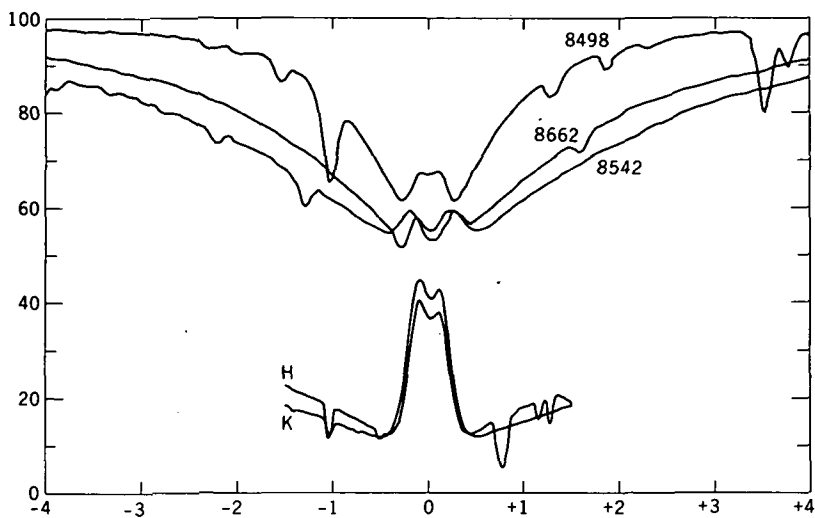


Figure I-21

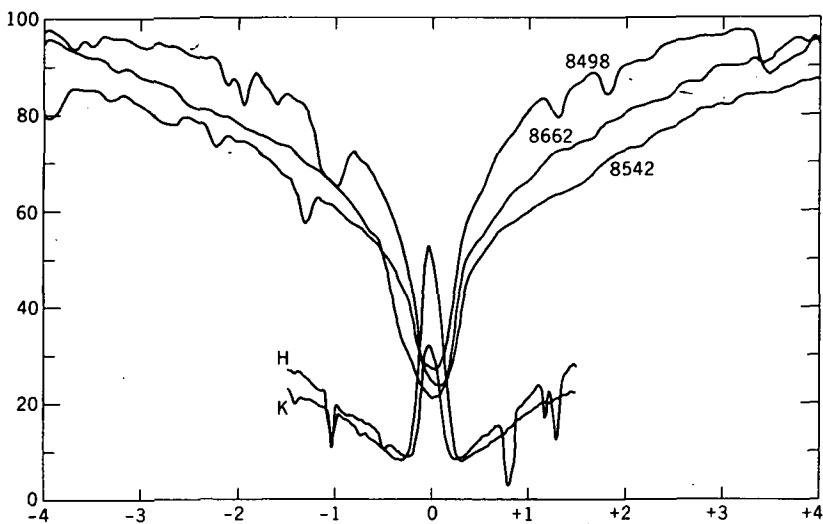


Figure I-22

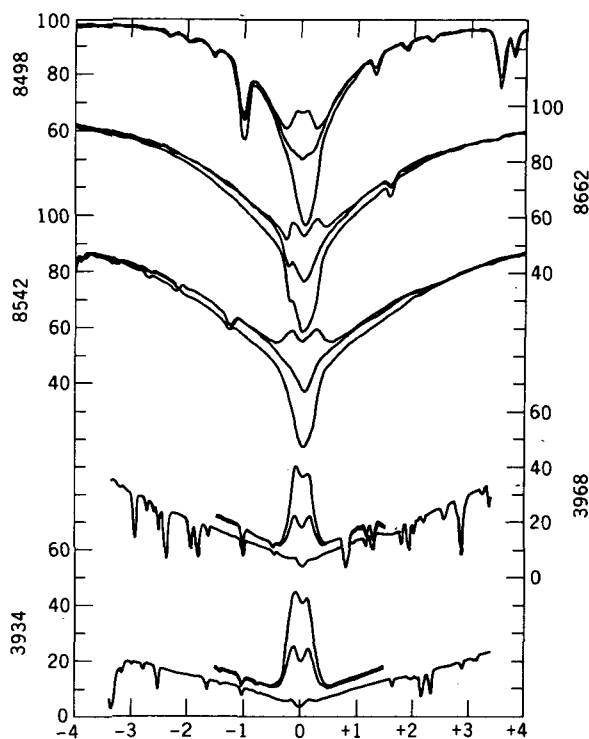


Figure I-23

significantly brighter than for the quiet profiles. This indicates that the plage phenomenon has an aspect which is photospheric and that the perturbation of the photosphere beneath a plage is independent of the chromospheric aspect of the plage. The sun thus exhibits two photospheres in addition to many chromospheres.

The main reason why I show these data before they are analyzed is to emphasize that the Sun has many chromospheres and that the calcium lines are sensitive indicators of these chromospheres. Clearly any acceptable theory for how stellar chromospheres vary with spectral type, luminosity, and age must explain the vast range of chromospheres on the Sun. To my mind this is an important example of why the study of stellar chromospheres and the solar chromosphere must be pursued together.

Cayrel — These observations are very challenging, as theoretical predictions are most often compared to average spectra. Yet, these data show

that we obviously have a wide range of chromospheric activity. Are there comments on this?

Underhill — This observation that the wings of these lines formed in plage regions have more flux in them than the same lines in the photosphere makes one wonder. I would ask Linsky, or any other theoretician, would this heightening of the flux from the deeper layers of the photosphere correspond to a back warming? One comes back to the problem that you cannot logically separate a photosphere and a chromosphere. They overlap. They react back on one another. If you have dense material overlying a radiating region, its going to produce back warming. We've seen a difference of about 2 percent in the energy coming out, and that's a back warming to me. It has more implications that just being one of those oddities you observe on the Sun. You would expect to find this on any star where there is an overlay of dense material. The result might be a totally different combined atmosphere. We may not think of line blocking and back warming in interpreting many ground-based spectra from A stars, B stars, even early F, but when you go to shorter wave lengths, there are a lot more lines, so you are going to get lots of back warming. These are strong resonance lines which are going to produce strong absorption in the outer fringes and which you might not even guess about by observing at 4000 Å. Are any theoreticians able to make these ideas more precise?

Pecker — I would like to comment in a slightly different way. Linsky has given us some beautiful examples of what Jefferies told us this morning, that the source function and the flux in the line are extremely sensitive to such things as density effects. His results illustrate that the effect of very small terms, as shown by Thomas and Jefferies years ago, is sufficient to produce large emission differences in the cores of these lines. From the shape of the source function, you can infer the shape of the line. What is important in the source function, then, are the source terms, even when they are small. For example, consider the difference between the polarization in the case of isotropic scattering and of a small perturbation on the isotropy. The results are significantly different. This is an analogous situation. I'm not sure that I'm replying directly to Anne Underhill's point, but I feel that the source term in the source function equation is the essential one in interpreting the observations Linsky has shown us.

Bonnet — I don't understand if you really assume that the differences in observations between the plages and the quiet regions are mainly due to a density effect? Is that correct?

Pecker — More or less. Yes.

Bonnet — How then do you explain a similar difference in the continuum at 2000 Å, where the difference is a temperature effect and not a density effect?

Pecker — I don't want to say it's a temperature effect or a density effect. I just want to point out that the effect of the smaller term on the source function is great, even though it's a small fraction of the source function. It's still sufficient to produce a tremendous difference in the flux in the central part of the line. In the photosphere we might have a different situation, wherein the temperature effect dominates. The density effect there may be absolutely negligible. What counts is source term. That's my main point.

Skumanich — I don't agree that the density effect is great. The source function is $\sqrt{\lambda}(B)$ at the surface. Now λ is proportional to the density N , and $B \propto T^4$ or 5 (for Cak), $\sqrt{\lambda} \propto \sqrt{N}$ while $B \propto T^4$ or 5 . Thus small changes in T are more important than small changes in N in influencing the central intensities.

Peytremann — Let me go back to backwarming effects from the chromosphere down to the photosphere. The backwarming effect cannot be very important because it should be considered as integrated over the entire spectrum, and the chromosphere flux is very small compared to the total photospheric flux.

Underhill — Are your remarks based solely on considering the backwarming from the H and K lines? You must consider all the other lines.

Peytremann — The lines formed in the chromosphere consist of the cores of strong lines, so they don't cover a wide spectral range. What is important to backwarming is the total energy integrated over frequency.

Linsky — I would like to comment on the question of whether the source function increases with density or temperature. One should consider the ratio of the residual intensities of K to H which increases with K emission in the Sun and, as Olin Wilson's work has shown, in other stars as well. In the absence of collisions K would be brighter than H where the temperature gradient is positive since the thermalization length for K is one-half that for H on a common optical depth scale. Fine structure collisions tend to establish equilibrium in the population ratio of the upper states of H and K. Thus the line ratio data on plages could be accounted for by either (1) lower densities or, (2) steeper temperature gradients, or both, in plages relative to quiet regions. The same argument applies to stars with active chromospheres relative to those with quiet chromospheres.

Cayrel — I do not understand how one can exchange density against temperature. How can you change the density without changing the scale height?

Skumanich — I would like to call attention to Domenico's work in which he asks what kind of parameter changes you must have in scale height and in temperature gradient. He found that the major effect which constrains the data (the observed K to H ratio, the observed amplitudes, and the observed half widths of the stellar Ca emission core) is the temperature gradient rather than the scale height. For example, a 33% increase in the temperature excess in the chromosphere of solar type stars will cover the whole range of Olin Wilson's observations.

Thomas — The parameters you have for the CaII H and K lines are the absolute intensity of the peaks, the ratio of K_2 to K_3 , the half-width and the position of the peak. If you give the temperature distribution as a function of depth, as we have shown a long time ago, the ratio K_2/K_3 is extremely sensitive to the place where the temperature rises in the chromosphere. The absolute intensity is extremely sensitive to the temperature in various regions. Elske Smith showed long ago that over sunspots, over plages, and over faculae the emission intensity rises up to various fractions of the continuum. What counts is the distribution of temperature as a function of optical depth, to which these things are extremely sensitive functions. And for that very probably the density comes in in a much different way than we are talking about here. Again in the same way, the magnetic field comes in, not because the magnetic field enters directly, but because the magnetic field changes in one way or the other the rate of deposition of mechanical energy that must be balanced against all the rest of things in the energy equation. So, is it sufficient to assume a distribution of temperature and density and ask what will come out of it? Do we not have to ask how the distributions of temperature and density are obtained? If the assumption of a frequency independent source function is wrong, the behavior of the K2 emitting region relative to the low photosphere could be in serious error. And the introduction of the microturbulence parameter to match the width of K2 may be suspicious.

Beckers — I would like to make a comment on the data presented by Linsky on the infrared plage profile. In Linsky's plage profiles the 8498 lines show self-reversal in the center, while the 8542 lines show a shoulder but no self-reversal in the center except where the plages are very strong. Those two lines have an absorption coefficient ratio of 1 to 9. This is a very large difference compared to the H and K lines. I assume here that the source functions are equal and that the levels are strongly coupled. The source functions for the three infrared lines are therefore equal.

I claim that the 8498 profile, because of its shape, must be formed near to the peak of the source function. The 8542 line has a much higher absorption coefficient and the line center therefore originates much higher in the atmosphere. The reversal therefore occurs in the wing of the line. If the source functions are equal and the absorption coefficients occur in the 1 to 9 ratio, then the intensities at the wavelengths where the lines have equal absorption coefficient should exactly correspond; the $\lambda 8498$ profile should be completely reflected in the $\lambda 8542$ profile so that the central reversal in 8498 should occur in the wings of the 8542 line. Why don't we always see that? Perhaps the spectral resolution does not allow one to see such a sharp peak in the steep line wing. Or perhaps, since one is working in the wing of the line, variations in microturbulence with height smooth the contribution function more than in the line center.

Athay — I have two comments. First, all of those questions are very easily answered on a computer in a few minutes. Secondly, I don't understand all of the concern about ten percent differences between H and K. We've been talking as though there were infinite coupling between the source functions. You don't get complete source function equality unless the coupling is very strong. It is probably very easy to get ten percent differences in source functions.

Underhill — I wonder if it would be helpful to broaden the discussion to another spectrum with a similar energy level distribution as CaII, namely that of BaII. CaII has an ionization potential of 11.87 volts and the lowest levels are 4^2S , 3^2D , 4^2P , etc. BaII has an ionization potential of 10.01 volts and there are equivalent 6^2S , 5^2D (metastable) and 6^2P levels. Have the solar people looked at the BaII lines? They are much weaker because Ba is much less abundant than Ca.

Aller — The abundance of Ba is about four powers of ten down from that of Ca.

Underhill — That would certainly make the two cases different.

Jefferies — Has anyone observed the CaII infrared triplet lines in stars other than the Sun?

Wilson — Paul Merrill and I did a little of that many years ago but I have no good data on it. I don't remember seeing any reversals in these lines.

Jefferies — Weyman and I made a very few observations of the infrared triplet but we certainly didn't see any reversals.

Cayrel — Of course, the stellar observations would not have sufficient spectral resolution to allow one to see such reversals even if they are there.

Underhill — Why not observe late type giants with a Fabry-Perot interferometer? That would work nicely at 8500 Å.

Linsky — I have some profiles of Procyon and Aldebaran which I will show tomorrow in the session on observations.

Steinitz — I would like to make another comment about the infrared triplet. I don't want to suggest an explanation for the differences between the behavior of 8498 Å on the one hand and 8542 and 8662 on the other hand. But just to complicate matters I would like to introduce the problem of the effect of Zeeman splitting on the source function. The 8498 connecting the $3/2$ to $3/2$ levels has a different Zeeman pattern than the other two lines. 8542 and 8662 have essentially the same Zeeman pattern with only a slight difference in the amount of splitting. The patterns are shown as follows:

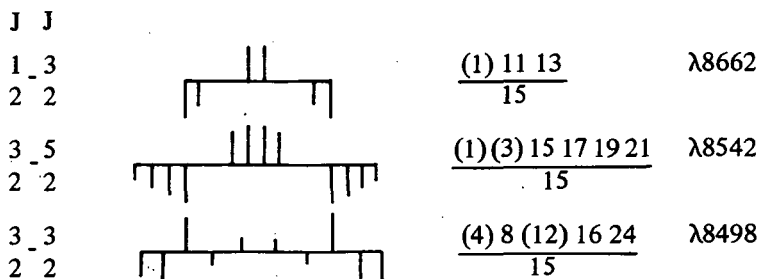


Diagram showing approximate Zeeman patterns for Ca II IR lines.

Now we know that plagues have a connection with magnetic fields, although I'm not suggesting that this is the ultimate explanation. But it may be necessary in transfer problems of this type to take into account these magnetic effects, especially since we see the nice differences. There is a ratio of about 1:5:9 in the intensities of the lines and these observations may be related to a difference in the slope of the source function as a function of optical depth. Another complication is that it has been generally assumed that the source function over the line is independent of frequency, the frequency dependence coming through the optical depth effect. That has been assumed because in the core of the line it is only fair to assume that there is equality of the emission and absorption profile, but when you take induced emission into account that may not necessarily be true.

Thomas — Steinitz is much too modest. His thinking is what has made me worry about the frequency independence of the source functions. I think he is giving us only a suggestion of the mechanism he is thinking about.

Peytremann — How strong must your magnetic field be so that the width of the Zeeman pattern competes with the velocity broadening?

Steinitz — I would guess about 1000 - 2000 gauss.

Sheeley — Assume the Zeeman splitting is 3×10^{-5} A/gauss, then 1000 gauss yields 0.03 A. I suspect that those peaks are located well beyond that.

Steinitz — But that is not the relevant point. It is not a question of whether the Zeeman broadening is larger than the velocity broadening. The question is what happens to the source function and how does the line core build up.

Thomas — The point Steinitz is trying to make is the following. Remember, in the source function I have a big radiative term plus a much smaller source term, the ϵB or the ηB^* , and in the denominator, unity plus again a sink term. A complete theory gives still another term in the denominator which results from a difference between the emission profile and an absorption profile. How big does the profile term have to be before it becomes important? It doesn't have to be big at all, because for CaII the largest comparable term is ϵ , the collision ratio, which is about 10^{-4} or 10^{-5} . So the disparity in the profile term must only be bigger than 10^{-4} or 10^{-5} to have an integrated effect big enough to affect the profile of the CaII line. If the emission profile and the absorption profile differ by one part in 10^4 or 10^5 the difference will be important.

Jefferies — I'd like to translate this discussion in case some are getting a bit lost. The problem concerns the preservation of frequency in the scattering process. Consider the absorption of a photon at a certain frequency and its subsequent re-emission. Is there any correlation between the frequency of absorption and the frequency of re-emission? The computed line profile depends very much on this question. The assumption generally made is that the frequencies of these two photons are entirely uncorrelated. Under those circumstances the line source function is not a function of frequency within the line. What concerns me about the arguments given here is the following. One of the infrared triplet lines (8498) is observed to have peculiar properties. When a photon in that line is observed the atom is raised to the $P_{3/2}$ level. What choices are then open to the atom? It can come down in the same transition, in another infrared line (8542), or in the K line. If it re-emits the same 8498 line photon there may possibly be some coherence in frequency between the absorbed and emitted photons. If the atom emits a photon in another line transition, then knowledge of the frequency of the absorbed photon will be lost even if radiative interlocking processes lead to a subsequent

re-emission of an 8498 photon (a process which could legitimately be called scattering). I agree that the profile of the 8498 line is peculiar and demands some sort of an explanation. I think that this is perhaps the most significant thing that came out of Linsky's observations. But I don't think we can explain this in terms of a partial coherence in frequency because the 8498 line couples so strongly with the other infrared lines and with the H and K lines.

There is one line I know of which may be an important candidate for a departure from the assumption of complete redistribution in scattering: namely, Lyman alpha. In this line most of the scatterings that take place are just direct absorptions and subsequent re-emissions going back and forth between the upper and lower states. It is, thus, not at all like 8498 where you get many sets of possible re-emission paths for an absorbed photon. It is interesting that Lyman alpha is characterized in the solar spectrum as having extremely extended wings which are in fact characteristic of a departure from complete redistribution.

Thomas — You're talking about the J scattering term. What I'm talking about is not the large number of scatterings but the differential effect which comes from a source-sink term. That's very small.

Jefferies — Yes. But you've got the intensities of a lot of different lines mixed in together in the source-sink terms. I don't think you can really argue on the basis of a two level source function for effects that are as sophisticated as this, or even an equivalent two level atom.

Skumanich — I want to make a plea. We have been talking about temperature and inferring from the temperature and the temperature gradient what the mechanical heating requirements are. I think one of the very important elements in this whole thing is calibration. As an example, Lemaire and I have compared the magnesium doublet emission with the O I lines at 1300 and we find that they don't compare well at all. (I mean compare by relating the data to some comparable quantity like the temperature distribution which gives you the observed shape as well as the amplitude.) They don't agree to such an extent that the calibration can be different between the two lines by as much as a factor of two, which I think is terrible. If we are after mechanical energy heating, one of the underlying questions we must all have in mind is that we need not only shape information on lines and continua, which is the classical thing astronomers have been doing, but in the new spectroscopy (to quote a colleague of mine) we also need absolute magnitudes, i.e., the absolute flux. So, I want to make a plea for not only careful and sophisticated theory but careful and sophisticated calibrations.

Ulrich — I'd like to ask Jeff Linsky just how firmly he believes in the wing difference of a few percent. I have to agree with Anne Underhill on this. I think that's one of the most significant things in these observations because that indicates a basic change in the thermal equilibrium of the photospheric layers. I feel this is of vital importance. Related to this I wonder if there isn't a similar enhancement of the continuum. If the continuum far from the core of the lines is also affected under a plage I think this would be extremely interesting. As Skumanich has emphasized, the results depend critically on the accuracy of the calibration.

Linsky — I trust the data on the enhancement of the calcium line wings in plages because spectroheliograms taken in the wings of these lines show bright plages and network out to about 10 Å from line center. Whether the continuum is enhanced or not in plages is a more difficult question that Neil Sheeley could better answer. I would not be surprised if there were a 1% enhancement at 4000 Å.

Athay — Isn't it true and well known that the continuum is brighter in a plage at least in faculae, that the faculae occur high in the photosphere and that they're more prominent in the active regions than they are elsewhere?

Bonnet — This is obvious in the UV spectrum. When you look at the Mg II lines you have the same mechanism and if you observe the continuum in wavelengths ranging from 2800 angstroms to 2000 angstroms you observe a strong enhancement of the continuum emission.

Sheeley — I'd like to make some comments about plages and continuum at the center of the disc at various wavelengths. We've made simultaneous spectroheliograms in the 3884 Angstrom continuum, which is the only continuum I can find in that range, and the nearby CN bandhead which shows faculae very pronounced. In the 3884 continuum a static photograph does not show brightenings in the continuum. But a time average or a movie of this does. It must be therefore a small effect but it's present. Ed Frazier has made some observations at Kitt Peak using a photoelectric magnetograph looking at the green continuum and finds an effect of about one half of one percent with the plages in the continuum being slightly brighter than average. Then there are some other confusing details such as if you take a spectrogram and look at magnetic field regions sometimes the continuum is brighter than average, but then sometimes the continuum is darker than average in the green. So while there are some details to be ironed out, time averages and high sensitivities do show a small possible effect.

Cayrel -- We should now conclude this part of our discussion on line formation. We had a specific question in the program, namely, what lines depend on the local physical parameters in a highly sensitive way. We should try to list those lines that fit the criterion, and then identify those lines that are not too difficult to compute. It seems obvious that the list includes the calcium H and K lines, at least for stars later than G0.

Thomas -- The answer, categorically, is collision dominated lines. Which lines are collision dominated depends on the star. You can't give specific lines for all stars.

Pecker -- This morning John Jefferies started to make a list of lines that are collisionally dominated and those that are photoelectrically dominated but which are classified in this way only for solar type stars. Are we able to make the same list for other stars at the present time?

Athay -- I want to raise an objection at this point. As far as I know no one has ever found a solar line that is really photoelectrically dominated. The sodium D lines are collision dominated. Even H alpha shows a strong measure of collisional effects. If you compute line profiles it's very easy to get emission cores in H alpha. In the case of every line we've ever computed it's easy to get an emission core if you simply increase the opacity of the chromosphere a bit or raise the temperature a bit. I just don't know of any line that is really photoelectrically dominated in the case of Sun. H alpha is supposed to be the prime example and is found to be a marginal case at best.

Thomas -- I can't say anything except that I completely disagree.

Athay -- A half a dozen people have published results that support the contrary opinion. If you disagree, please publish it.

Thomas -- It was published, as you well know, a long time ago.

Athay -- And it's been shot down and you haven't replied.

Thomas -- No. There isn't a single case of an H alpha profile except in a place like a flare which shows some indication of a temperature gradient.

Athay -- The central intensity itself shows it. The only reason that the temperature gradient shows up in the H and K lines is that they are the only lines that have enough opacity to show it.

Cayrel -- Yes. That was the second point I was going to raise. It's not enough of course to have a line with a sufficiently large collision rate but you must also have a thermalization length as large as the region where the temperature increases. This double restriction is perhaps why we have so few lines to work with. It is regrettable that we cannot discuss at the same time hot stars and G stars because the conditions are so different.

Thomas — It seems to me this is the big point. This is a symposium on stellar chromospheres. What we are trying to do is to see physical principles on the basis of which we can proceed.

Cayrel — Yes. Now we should select particular lines for different classes of stars. The CaII infrared triplet is somewhat sensitive to a chromosphere but to a lesser extent than the H and K lines. On the contrary the resonance lines of Mg II at 2800 Angstroms are on the whole much more sensitive to a chromosphere. I don't know the order of magnitude but Bonnet can certainly comment on the comparison between Ca and Mg H and K emission.

Bonnet — The measurements made by Lemaire of the Mg II doublet emission show that the contrast between the maximum emission in the lines and the adjacent continuum varies from 25% to 40% at the center of the solar disk.

Cayrel — We must also be very careful to indicate what spectral resolution is needed in order to see the central emission in sufficient detail. Could I ask first, what resolution is necessary to distinguish the separate emission peaks with acceptable accuracy and second, what resolution is necessary just to show that there is some central emission — both for Ca and Mg H and K?

Bonnet — For the sun this resolution can be estimated to range between 0.1 Å and 0.2 Å.

Athay — I would like to make a suggestion that we ought to look at the Fe II resonance lines. We're now talking about an iron abundance that is just as high as that of Mg and just as high as Si. The published f values for the lines are also just as high as for Mg and so, just on that basis, you would predict that the Fe II resonance lines ought to be just as strong as the resonance lines of Mg II. However, it is clear from looking at the rough spectra we have that this is not true at all. The Fe II are very much weaker than the H and K lines of Ca, but if there is as much Fe II as some people say, (and as I believe there is) then there's just no reason why these lines should not also show self-reversals.

Thomas — What about the Boltzman factors for these ionized lines?

Cayrel — And is not the partition function of ionized iron rather large?

Athay — You put all the Boltzman factors in and you still predict lines as strong as those of Mg, even with only a fourth of the ionized atoms in the ground state?

Thomas — I would like to comment on a related matter. Noyes and Kalkofen have produced a model atmosphere of the sun coming from the Lyman continuum analysis. If you remember, this model is strikingly similar to the one we had in that book of ours a long time ago. There we made the same kind of a model on the basis of an analysis of the free-bound and the H-emission in the solar atmosphere. All that depended very carefully on being able to determine b_1 and b_2 of hydrogen. The basis of that determination was that the n_2 and the n_3 levels were fully ionization controlled; so that there is a large population of the n_2 state throughout the atmosphere, and also that H alpha was photoionization controlled, so that one could make a correction to the ionization equilibrium coming through the presence of H alpha. The Noyes and Kalkofen model essentially agrees with ours. So now if you believe this current model of the solar atmosphere you have to believe that H alpha is photoelectrically dominated.

Athay — All that says is that we were approximately right.

Thomas — Kalkofen, in your ionization equilibrium calculations, don't you find that the ionization terms are the dominant ones?

Kalkofen — It is true that the most important transition upwards from the second level is by photoionization.

Thomas — OK. That's the thing that controls the population.

Cayrel — I presume that this discussion is still related to iron, in which the interlocking terms may be more important than in hydrogen or calcium because of the greater complexity of the atom, hence; many more possibilities beyond the $1 \rightarrow 2 \rightarrow 1$ process.

Underhill — There are some interesting peculiarities because some of the Fe II lines go into emission before you cross the limb. Somebody mentioned these lines earlier in the day. There are quite a few such lines in the solar spectrum, for example, Ce II and other rare earths. However, the Fe I lines apparently do not have this behavior.

Cayrel — I would add to this list the lines of the type suggested by George Wallerstein, forbidden lines in which C_{21} is much larger than A_{21} . The point was raised that the C_{21} should then be also larger than other competitive transition probabilities, so that we are sure that the source function is really the Planck function. One point is that these lines are never as strong as the permitted lines, and that they do not allow you to reach very high in the chromosphere.

Pasachoff — I have some Sacramento Peak spectra that show the resonance line of Sr II going into emission slightly inside the limb. Nearby

are various rare earth lines including mostly Ce II. They are also in emission inside the limb, which is well known since Menzel's work and they are in emission further inside the limb than the Sr II seems to be.

Cayrel — The problem of Zeeman splitting has been raised which may make the whole theory described this morning by Jefferies more complicated, if one wants to take into account redistribution due to changes between Zeeman components. It should be pointed out that the Zeeman splitting is much less of a problem for H and K than for the infrared triplet lines. This should be true for Mg as well as Ca.

Johnson — May I add Na D to this list? Since its source function is collisionally dominated (an exception to the rule mentioned), it may be sensitive to a temperature reversal. Also, whereas these other lines may be weak in cooler stars, Na lines are extremely strong, and are sometimes used as luminosity indicators. Does anyone know of observations showing emission reversals in the cores of these lines in cool stars or the Sun?

Underhill — They appear in emission in a few peculiar hot stars.

Sheeley — I think that this may be a matter of height of formation more than what the particular energy level scheme is. Spectroheliograms in many lines such as the core of the Na D lines, Sr II, Ba II, Sc II, Fe II . . . (all strong lines) the core of Mg I b lines, the Ti II resonance lines at 3349 and so forth all look similar. They fall into a special class of their own. This business of classifying isn't too unreasonable since you can get the same sort of classes that Jefferies got this morning . . . for example, from the same approach. So, I think it's a matter of where the lines are formed. The classes that Jefferies indicated are formed high in the atmosphere. All these other lines (Fe II, Sc II, Ti II, etc.) are formed in the intermediate chromosphere. And in the lower chromosphere or the upper photosphere, whatever you want to call it, there is another class of lines and molecules — neutral iron lines, neutral metals in general, and so forth — which also show very bright plages as for example CN shows. The CN bandhead at 3883Å shows faculae that are brighter at that height in the atmosphere than even the K line. The K line has a contrast of say 50% in the lower chromospheric faculae ($\Delta\lambda \approx 3\text{\AA}$) whereas the CN bandhead has a contrast of 100%. So perhaps CN is worth looking at in stars.

Pecker — By all means we should look very carefully at molecular lines, but primarily for very cold stars.

Underhill — No, the molecular lines, in particular CN, are very important in moderately hot atmospheres. Consider the flash spectrum of the Sun. You can look back to the 1930 list of lines in the flash spectrum by

Menzel and some of the most prominent are due to CN. They're low chromosphere lines even though they are molecules and they are formed where the temperature may be 8000 degrees. When you say cool stars and molecules, you may be thinking 3000 degrees or less. CN arises at twice such a temperature and I think CN is a very important intermediate temperature indicator. The reason I say that is because of the well-documented presence of CN in the flash spectrum, which is definitely chromospheric.

Pecker — I completely agree with you. I just wanted to stress the fact that so far this is the first time a molecular line has been mentioned today. And that we shouldn't forbid the molecular lines to enter into our analysis.

Boesgaard — I want to add to the list of lines the Fe II lines discovered by Herzberg in M stars and found in an MS star and in Carbon stars. There are 17 lines in the region 3150-3300 Å from multiplets 1, 6, and 7.

Cayrel — Can you observe these from Mauna Kea?

Boesgaard — Mauna Kea is one of the best observing sites because of the high UV transparency at 14,000 feet. However, these cool stars are not emitting very much in the continuous background in that wavelength range so the exposure times are long.

Cayrel — I am surprised that nobody has mentioned the He 10830 line.

Beckers — The helium lines are very strongly radiation dominated. If there is any line that is not collisionally dominated, it is this line.

Sheeley — At Kitt Peak, Giovanelli, Harvey and Hall have taken some very nice spectroheliograms in 10830 with high spatial resolution. They look very similar to, although not exactly the same as, H alpha. 10830 would fall in the same category as H alpha, H beta, gamma and so forth.

Cayrel — But it is an absorption.

Sheeley — Yes.

Cayrel — We don't worry too much about what kind of source function we get in this line as long as we detect it is absorption. The attractive thing is that you can observe it in hotter stars if it exists, without having a bright continuum masking a weak emission line.

Linsky — Another helium line that appears prominently in absorption in strong plages is the D₃ line at 5876 Å. This line certainly indicates a chromosphere and should be looked for in solar and later-type stars. I would like to point out that the CN bandhead at 3883 Å is a very interesting spectral feature to study. A detailed non-LTE analysis of the

violet system of CN will not be easy, but the bandhead should be sensitive to temperature at the temperature minimum and above for stars like the sun and somewhat later. Since the CN bandhead consists of about five overlapping lines, it is essentially a piece of continuum and thus insensitive to broadening, velocity fields, and magnetic fields. Spectroheliograms taken by Neil Sheeley in the CN bandhead show great contrast between bright and dark regions and appear to show fine structure in the chromospheric network quite well. George Mount, a graduate student, and I are presently studying CN spectroheliograms and center-to-limb photoelectric data in an effort to understand what the spectroheliograms are telling us.

Pasachoff — I should say that I am now working on a continuing program of observing D_3 lines in late type stars to look for stellar chromospheres. I think that a report is better fitted for the discussion tomorrow morning. It is a tricky line to detect and there are some atmospheric lines in the region so it is not just a matter of looking for it and finding it. The original work done on the D_3 line was by Wilson and Aly, published in the PASP in 1956 (68, 149) in which they reported finding a line near the D_3 wavelength in several stars. The M star spectra are too complicated to tell whether a line that falls at that wavelength is the D_3 line or not. Since that time Vaughan and Zirin (Ap. J., 152, 123, 1968) have published results of their extensive observing program and Zirin is continuing a program on 10830 with the 200-inch telescope. They published many equivalent widths of 10830 lines both in emission and in absorption in late type stars, finding some that even seem to vary in intensity. In my search I had the benefit of knowing which stars, such as λ Andromeda, have a lot of 10830 in them. One way we can tell the origin of lines that we see at the D_3 wavelength is whether the intensities correlate with 10830. I should point out to people here who are calculating models that it would be of great interest to have more detailed models for the He lines, in particular the expected intensities and ratios of equivalent widths of 10830 and D_3 for various kinds of stars of type F, G, and K.

Fosbury— M.W. Feast (M.N.R.A.S. 1970, 148, No. 4, 489) reported, in a paper on Lithium Isotope Abundances in F and G dwarfs, seeing $\lambda 5876$ in absorption in an F8 dwarf. The star is Zeta Doradus and is slightly peculiar in several respects. It lies slightly above the main sequence ($\Delta M=0.6$) and Feast measured a higher Li^6/Li^7 ratio than in any of the other stars in his program. It also shows unusually strong H and K emission for its spectral type. Wesson and I have looked for the $\lambda 5876$ line in some later type giants; we have also had discussions with Griffin and looked at some of his very fine high dispersion tracings. We could not

be certain of an identification in any of our samples. Figure I-24 shows the He I $\lambda 5876$ line in three spectra of Zeta Doradus. (Original inverse dispersion 13.7 Å/mm. M.W. Feast)

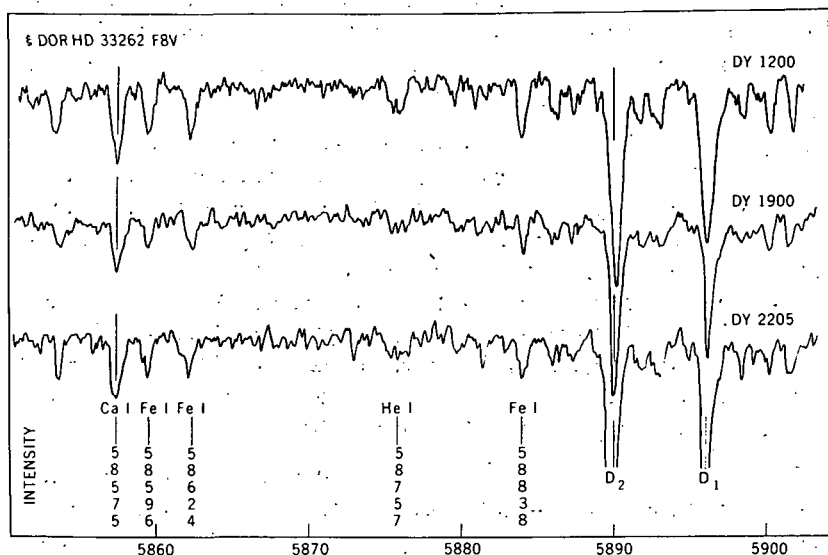


Figure I-24

Cayrel — Can we now give the name of a line in a hot star (a B star) which is the best case for detecting a chromosphere if B stars have chromospheres. Is anyone ready to answer this question?

Pecker — I'm not ready to answer this question, but this goes back to the discussion that Jefferies made about the geometrical emission properties and the real, true emission properties of a line.

Cayrel — If you are in a geometrically thin layer in which you have a temperature that is significantly higher than the boundary temperature that you predicted from a model in LTE, how will you detect that? I think that the distinction into two classes by Thomas is not the real point, because the collisional rate is certainly large for most lines, because the electron density is high when hydrogen is ionized. I refer here to hot stars.

Thomas — I disagree. I really think what you want to do is look at the very recent calculations Mihalas has been doing on this distinction between the photoionization dominated lines and collision dominated lines. He's imposed the conditions of radiative equilibrium but it's easily

generalized to the case where you have a chromosphere and lots of Mg lines, lots of Ca lines, although not Paschen alpha. He has very specific results on this.

Cayrel – I don't doubt that you can find lines that are collisionally dominated in hot stars, but I doubt whether there are lines strong enough in the visible spectrum, so that you could detect a chromosphere if it is not geometrically thicker than in the Sun. That's the problem.

Jennings – I would like to comment on the shell properties. I think if you make a distinction between stars with shells and stars with chromospheres, you're going to run into trouble among the late type stars. I would cite as an example alpha Orionis, which is certainly a late-type star with a chromosphere, since it has CaII H and K, as well as Fe II, in emission. On the other hand, from the work of Deutsch and Weymann, there is certainly evidence for a very extended atmosphere involving mass loss. So here we obviously have a chromosphere co-existing with a very massive shell; and so I would argue that one would have to be careful in dividing stars into those having only a shell or only a chromosphere.

Underhill – They're not mutually exclusive; the shell is never accurately defined for B stars. To add to the list of lines, I would guess that for the middle B stars the Si II lines are important. It is well known observationally that 4128, 4130 change their intensity relative to the red Si II lines 6347, 6371 which are from simple levels, are well behaved and are associated with the other multiplet at 3856Å. Now this has never been explained, though it has been observed. You never know whether the 4128 and 4130 lines are going to be strong or weak. The f values have been calculated by detailed configuration-interaction calculations. They've been observed and we know pretty well what they are with respect to other lines. Anyway you can't count changing one multiplet very much in one star and blaming it on f values. So the only thing that is left is the effect of chromospheric conditions. You have to compare the 4128 and 4130 lines with the red multiplet and the violet one.

Cayrel – But they are very weak.

Underhill – No. They're quite strong. The other lines will vary in intensity as 4138 and 4130 vary. They come from a 3^2D level and 3^2D levels always cause you trouble.

Thomas – There's one more thing. We've been concentrating here as though what you need to do is take a line such as H or K whose profile somehow tells you the existence of a chromosphere. But just as the 10830 line in the Sun indicates for you that there's a chromosphere simply because you see it, so does any line in a hot star which should not

be produced under conditions of radiative equilibrium; for example, lines of O VI in the Wolf Rayet stars, tell you that there is either a chromosphere or a corona. Since listening to Kuhi this summer I am convinced that Wolf Rayet stars have coronas rather than chromospheres, but I think the thing one should put here as an indicator of the presence of chromospheres and coronas are ionization levels. Simply the presence of any lines, no matter how they are formed, which you would not observe under radiative equilibrium in that star indicates a chromosphere. For that reason it is absolutely essential that we have good ideas of upper level limits of temperature such as Auer and Mihalas have been calculating. We need to know the highest temperature levels you would have under radiative equilibrium.

Cayrel — I think it is time to end the discussion on lines. At least we know how to raise interesting problems for theoreticians. For example, someone should determine what happens with Si II in hot stars and see if these lines are really collisionally dominated and if the optical thickness is large enough to indicate a chromospheric temperature rise. I would now like to turn the discussion to continua and ask what are the good continua that indicate a temperature rise in the surface layers of stars.

Underhill — I would like to stress the importance of continua as chromospheric indicators. If you think about the long wavelength region around 8000 Å where H^- comes to a maximum you have one sort of opacity pattern. If you heat the atmosphere up to a temperature of 12000° or so instead of 7000° the opacity pattern in this spectral range changes its shape considerably, and free-free becomes one of the more important sources of opacity.

It has a different shape than H^- . That means your lines are going to fight against a different opacity, and it will change your relative intensities in that region. Therefore, there is the possibility of the continuous source changing, whether the star has an extended atmosphere with a temperature that goes down or goes up. Continuous opacity is an important indicator in regions where there can be differently shaped continua corresponding to a change in temperature.

Pecker — I agree with Anne Underhill; the Paschen discontinuity is important in hot stars, and there is a strong relation between it and the H^- opacity.

Jefferies — Perhaps the source function is not always the Planck function. If the absorption coefficient is decreasing toward longer wavelengths and if the radiation temperature is decreasing toward longer wavelengths, then you probably have a case for saying that the temperature is increasing upwards — this is the sort of thing Mme Gros will talk about in the

session tomorrow. However, when you get into regions where the hydrogen continua dominate you might have good reason to question whether LTE is the correct description for the source functions.

Underhill — When you get into the hot stars you may have a hot chromosphere starting at 50000K, then a high radiation field from 300 to 500Å. If you have radiation from such continua, this is going to affect the rest of the atmosphere. What sort of criteria could we suggest to look for? Lines in these spectra might serve as criteria for the presence of a chromosphere.

Pecker — Jacqueline Bergeron has computed several early type star models with a corona to explain the IR spectra, and the heating of the HII region which is outside the HII region surrounding the star.

Cayrel — Can anyone propose continua or lines in the visible as a diagnostic for hot stars?

Peterson — Hot stars have strong metal continua, particularly carbon continua primarily in the UV.

Peytremann — I would object to the continua since they are hidden by lines. UV spectra show that you never see a nice absorption edge. They are washed out by the high density of lines.

Underhill — Continua with no lines are the only ones that can be used. There are too many lines from 912Å to 6000Å from average stellar spectra to do much with the continua.

Sheeley — Where no energy is put into the spectra, it doesn't really matter, I would think the lines would have a negligible effect.

Underhill — Look between 3000 and 4000Å. There are so many lines no one knows what to do. In a paper by Houtgast and Namba a couple of years ago, In BAN they found between 40 and 50% line blocking, which is quite a bit. Line blocking can alter the spectra in these regions considerably.

Cayrel — From the viewpoint of models, is the continuum brightness temperature sensitive to the chromospheric temperature?

Cuny — Yes, it is sensitive.

Kalkofen — You couldn't use the Lyman continuum as a chromosphere indicator for stars earlier than B.

Thomas — From the HII region I can observe whether or not I have a chromosphere-corona. The HII region is a big part of the stellar atmosphere.

Underhill — Don't forget that we use the planetary nebula to tell us what the nuclei are producing in the way of flux. One of the best photon counters is a planetary.

Aller — Are you sure it is strictly a photon counter and that the emitted radiation cannot sometimes be enhanced by energy imparted by a stellar wind?

Underhill — The gas is moving, and there is mass motion, but it's still a photon counter, a gas flow counter. Now, for cooler stars, is there anything else we can use for a photon counter?

Pecker — I just want to object to what Anne Underhill just said. Is a planetary nebula a real good photon counter, or is it a counter of only detected photons? The Zanstra mechanism shows that Te in a PN is sensitive to the *quality* of the radiation, not to its *quantity*. The state of ionization, to the contrary, in an HII region, is a function of quantity of UV photons. So the sentence of Anne's is ambiguous, and should be used with a great deal of caution!

Linsky — One potential indicator of chromospheres in very late type stars, which has not been mentioned, is the pure rotation band of water vapor in the region of 20μ and longer wavelengths. Many very late type stars exhibit infrared excesses at 20μ , which have been interpreted as circumstellar emission. An alternative explanation is that the pure rotation band is sufficiently opaque that the region of formation of the band is in the lower chromospheres of these stars.

Jennings — I would like to comment on the H_2O . Even though water may have bands at 20μ , it is difficult to explain the strong features at 10μ , and it should be pointed out that various people have suggested silicates which have 10 and 20μ peaks. A number of investigators have discussed the shape of these peaks, and find that molecules cannot reproduce it while grains like Mg and Fe silicates can.

Johnson — Besides the spectral feature already discussed, there is another class of observations that might indicate stellar chromospheres. Spectral lines in late type stars often appear to be broadened by very large turbulent velocities (sometimes supersonic), and there are displaced lines in other stars that show outflowing material. In these stars we thus see evidence of energy dissipation or matter flowing from the photosphere, both of which phenomena we might call chromospheres.

Vernazza — We determined an empirical solar chromosphere model by assuming a temperature as a function of height and solving the hydrostatic equilibrium, statistical equilibrium and the radiative transfer equations for a 4-level H atom, an 8-level Si I atom, an 8-level C atom, a

5-level Ca II atom and H-, to obtain the continuum emergent intensity at any wavelength. T_e vs. height is adjusted until agreement with the observations is reached. As a result we are able to match the observed solar continuous spectrum from 500Å to centimeter wavelengths, as well as several lines such as Ly α Ly β and H α . From the model, which also includes a microturbulence structure, we can determine approximately the radiative energy losses at every height and every continuum frequency, as well as the losses in some of the hydrogen lines. I will give a brief summary of how the temperature model shown in Figure I-25 is adjusted. Essentially, the T_e vs. height model begins in the upper photosphere, extends through the temperature minimum at 500 km above $\tau_{5000} = 1$, through a quasi plateau in the chromosphere and finally through a high temperature plateau between 2000 km and 2200 km in the transition region. The temperature minimum is put at 4100 K. The first quasi-plateau is around 6000 K and the second at roughly 20000 K. In the photospheric region between the temperature minimum and 5000 K the temperature structure coincides with the H.S.R.A. Below 5000 K our model has a lower temperature because we solve the non-L.T.E. problem for H. The departure coefficients from L.T.E. for H are less than one, which gives a higher electron density than in L.T.E. As a result we have a

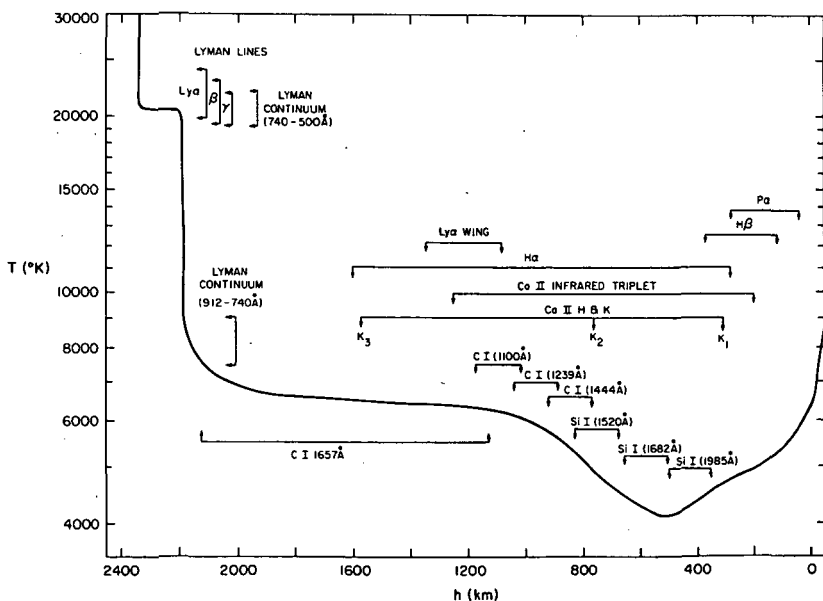


Figure I-25

lower T_e , but we nevertheless compute the emergent infrared intensities as they are observed. In the region of the temperature minimum the Si I 3P , 1D and $1S$ continua are formed. These continua serve to give us a good hold on the temperature structure at the temperature minimum. Until recently all realistic solar models have obtained the Si I continuum intensity in L.T.E., (except for some preliminary work by Y. Cuny) and required a higher T_e to explain the U.V. observations. Since we have a non-L.T.E. Si I solution the temperature can be lower because the Si I ground state source function is larger than the Planck function. This is due in part to the interaction between the Si I 3P continuum and the Ly α line. Since the Ly α line has a higher source function than in L.T.E., it controls the Si I continuum source function. The C I 3P , 1D and $1S$ continua are formed above the temperature minimum. These continua provide information about the temperature distribution at around 6000 K. The observed continuum intensities between 1440Å to 912 Å are reproduced by the present temperature model. In addition Ha H β and Pa which are formed over an extended chromospheric region are also reproduced. At around 8400K the Lyman continuum is formed. Above, in the 20000K plateau the Lyman lines are formed. There are several reasons for the existence of this small plateau at 20000K. One of the best observations we have is the ratio of the Ly α , Ly β , Ly γ , Ly δ , and Ly γ to Ly δ integrated intensities.

In order to satisfy these observations we need to have the 20000K temperature plateau. We know the Lyman continuum is formed at approximately 8400K. So above the Lyman continuum formation region, we are forced to have a very sharp temperature rise. Otherwise the optical depth in the Lyman continuum will be too large, and will be formed at a much higher temperature. Then somewhere at 20000K the temperature gradient must flatten to the point of producing a plateau to reproduce the Lyman line integrated intensity ratios and their absolute intensities. At the same time the plateau is necessary to obtain the central reversal in Ly β that, otherwise is impossible to obtain. Unfortunately there is only one observation of Ly β . The only way we have to reproduce the Ly β profile is by having a Ly β source function which decreases toward the surface. And the only way to obtain this decreasing source function is by means of a plateau. In addition we have center-to-limb observations of the integrated intensity of Ly α , Ly β , Ly γ , Ly δ , and at six wavelengths in the Lyman continuum.

The limb darkening observations are not good because inhomogeneities, namely spicules or dark mottles could introduce additional darkening, and by how much we do not know. That is the reason we do not rely too much on limb brightening or darkening observations. Lyman α has strong

limb darkening, about 75% of the Sun's center. Most of this XUV data comes from the Harvard OSO IV and VI experiments as well as from some unpublished rocket data from H.C.O. With this temperature structure we can compute the energy losses in the chromosphere. We have to keep in mind that these are still provisional results. In Figure I-26 the solid line represents the radiative energy loss as a function of depth for the present temperature distribution. The Lyman α contribution is shown by a short dashed line, Ly β by a long dashed line, H α by a dotted line, and the Lyman continuum by a dashed-dot line. In the upper chromosphere Ly α is the main cooling agent, while in the low chromosphere H α is responsible for most of the cooling. There is a diffusion of Ly α photons from the upper chromosphere into the low chromosphere. This produces some heating in the low chromosphere. The continuum losses are negligible except by some CI continuum cooling around 5500K.

Delache — I would like to ask if this 20000° Lyman plateau exists because of mechanical energy deposition in this region, or because the radiative losses have to occur in Lyman α .

Vernazza — We have calculated these loss curves from a temperature model which has been chosen empirically in such a way that the predicted spectrum agrees with the observed one. Then we have deduced the radiative gains and losses in order to determine the mechanical energy input necessary to maintain the temperature model.

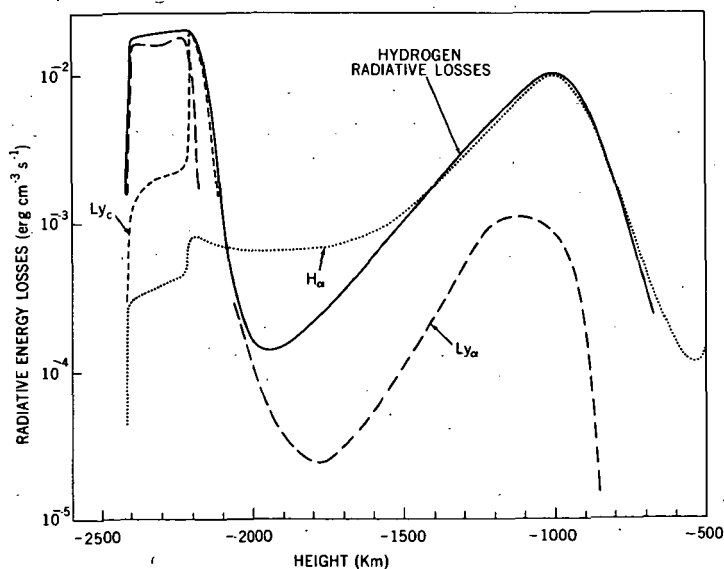


Figure I-26

Jennings — The loss rates should be proportional to the area under the curves you have drawn for Lyman alpha and H alpha. Do your results imply that Lyman alpha is giving up the largest part of the chromospheric energy loss?

Vernazza — Yes.

Skumanich — In addition to these results based on the divergence of the radiative flux you might find it interesting to compute the contribution of the divergence of conductive flux.

Vernazza — I understand that for a temperature of 10000°, Ulmschneider has computed the conductive flux coefficients in L.T.E. Given the extreme departures from L.T.E. I would be reluctant to base the conductive flux contribution on such results.

Ulmschneider — Using the temperature distribution determined from the Lyman continuum observation (Noyes and Kalkofen 1970, Solar Physics, 15, 120) one can compute the conductive flux. One finds that this flux is about $2 \times 10^3 \text{ erg/cm}^2 \text{ sec}$ compared with the observed radiation flux of about $6.4 \times 10^3 \text{ erg/cm}^2 \text{ sec}$, (Friedman 1963, Ann. Rev. Astr. Astrophys., 1 59), the difference being due to mechanical and radiation heating. The amount of radiation heating through the absorption of Ly α and Ly β photons in this region between the Ly continuum and Ly α emitting regions appears now to be crucial for the existence of a temperature plateau. This may be seen as follows.

The radiative loss in the Ly continuum, Ly α , Ly β regions is balanced by 3 competing heating mechanisms, thermal conduction, mechanical heating by shock waves and radiation heating. Of these mechanical heating becomes unimportant at greater height because, first, the increasing sound speed increases the wavelength, decreasing the strength of the shock wave and thus its dissipation, second, the dissipation of shock waves is a slow process and can not rapidly balance strongly increasing radiation losses. If radiation heating were also unimportant then thermal conduction would be the only significant heating mechanism. In the Ly continuum region the coefficient of thermal conductivity K, due to the increasing degree of ionization, is a decreasing function of temperature or height.

$$\frac{d\pi F_{\text{Rad}}}{dh} = \frac{d}{dh} K \frac{dT}{dh}$$

Thus through this equation any radiation loss and even zero radiation loss would lead to an increase of the temperature. This argument is especially valid in the main Ly α emission region. In this region we expect a strongly rising temperature due to thermal conduction.

On the other hand if radiation heating is appreciable then it could decrease the conductive flux leading to a temperature plateau between the Ly continuum and Ly α emitting regions. For example if a radiative flux of Ly α photons going toward the sun of about $2 \times 10^3 \text{ erg/cm}^2 \text{ sec}$ were absorbed in the region between Ly continuum and Ly α emission then assuming, for example, no emission in this region one could get

$$\frac{dT}{dh} = 0$$

as seen from the integrated version of the previous equation.

(note added in proof:) A numerical check of the importance of this Ly α back heating was done after the conference by W. Kalkofen. He found that it invariably occurred in various different models so that the existence of a temperature plateau seems to be fairly certain although for reasons different than originally proposed (Thomas and Athay 1961, Physics of the solar chromosphere. Interscience, New York. p. 156).

Vernazza — (Note added in proof:) I referred to the conductive flux coefficient published by Ulmschneider (Astro & Astrophys. 4, 144, 1970 which is calculated assuming L.T.E. Later, however, Ulmschneider kindly provided me with a more general conductive flux coefficient subroutine. The divergence of the conductive flux was calculated and was found to be insignificant.

PART II
OBSERVATIONAL EVIDENCE FOR
STELLAR CHROMOSPHERES

Chairman: Leonard Kuhi

INTRODUCTORY REMARKS BY SESSION CHAIRMAN KUHI

Today we would like to discuss the basic observational facts relating to the detection of chromospheres in the Sun and in other stars. Francoise Praderie will be talking to us about the solar and stellar data obtained from ground-based observations in the visible and infrared. Afterward, Lowell Doherty will discuss the ultraviolet data.

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EVIDENCE FOR STELLAR CHROMOSPHERES PRESENTED BY GROUND-BASED SPECTRA OF THE SUN AND STARS

Francoise Praderie
Institut D'Astrophysique, Paris

INTRODUCTION

NEED FOR A DEFINITION OF A CHROMOSPHERE

Before starting to survey recent observations related to stellar chromospheres, an operational definition of a chromosphere is needed; such definition must satisfy two requirements: (1) it must be bound to a set of *observables* which we agree indicate the presence of a chromosphere; (2) it must be reasonable in terms of the *physical effects* which we say characterize a chromosphere. Indeed one does not want to be *a priori* confined to call chromospheric indicators only those spectral features which, in the Sun, have been attributed to the chromospheric regions of formation of the spectrum, and which, by analogy, can be said to be a sign of a chromosphere in stars similar enough to the Sun.

The superiority of the Sun lies in the fact that a correspondence has been established between chromospheric observables and the chromosphere as a physically defined layer of the atmosphere; a combination of both very detailed observations and a refined theory of spectrum line formation have made this correspondence meaningful. Consequently, a safe way to proceed, at the moment, would be to study stellar chromospheres as examples of solar type stars. This approach, although good if the aim is to give a quantitative description of solar-like stellar chromospheres, excludes many stars with "anomalous" spectral features; those features do not necessarily have a counterpart in the Sun's chromospheric spectrum, but nevertheless suggest that the stars showing them have an energy supply due not exclusively to radiation in their outermost layers. For the latter stars, our diagnostic tools are still poor, and this will prevent us from giving any but qualitative descriptions of their chromospheres. While we must here look at the Sun as a typical example, about which we know more because of better observations, and which will therefore serve as a guide, we will try to classify (but not to interpret in full generality) observed features pertaining to stellar chromospheres in the definition of a chromosphere based on energetic considerations.

LIMITS OF AN EMPIRICAL DEFINITION

The preceding section implies that we already have in mind a representation of what the solar chromosphere is, both in terms of observables and in terms of physical effects. Concerning the observables, we know empirically what is the chromospheric spectrum of the Sun as observed at eclipses, and what are classically called the solar chromospheric layers, i.e. those extending from $\tau_{\text{tang}}(5000 \text{ \AA}) = 1$ to $\tau_{\text{tang}}(\text{H}\alpha) = 1$. Further out, in the Sun, lies the corona. But clearly we have said nothing regarding the physical effects which define a chromosphere by locating it in terms of tangential optical depths. Moreover this last variable is not accessible in the majority of stars (except in eclipsing systems, e.g., $\xi \text{ Aur}$). As a matter of fact, when starting to interpret empirical features in the solar chromospheric spectrum like emission gradients, or intensity reversals in H and K lines, one recognizes primarily that not optical depth but electronic temperature T_e is the basic physical quantity which contrasts a chromosphere relative to a radiative equilibrium (RE) atmosphere; T_e describes the energy balance and its departures from the pure RE case.

What we ideally want then is to give a unified definition of a stellar (including solar) chromosphere, thereby avoiding a purely empirical one, and relating it to the physical effects controlling T_e . From this standpoint, the atmospheric regions above the photosphere are combined, and in the following discussion there will be no need to separate chromosphere from corona. Only the problem of the base of the chromosphere will be treated, not that of its top.

TENTATIVE DEFINITION OF A CHROMOSPHERE

We suggest that the chromosphere is the region of the star giving rise to *observables* depending upon the existence of a) a mass flux, b) a non-radiative energy dissipation. Two questions immediately arise: first, why link the existence of a chromosphere to both phenomena a) and b) and not simply to b) and, second, what kind of observables are indeed chromospheric indicators? We now turn to consider the necessary and sufficient conditions for a chromosphere.

In a star, considered as a non-equilibrium system, motions are produced in the subphotospheric or in the photospheric regions from the electromagnetic energy flux, through various instabilities. In the contracting envelope of a protostar, mass falls toward the center of the cloud. In both cases, any motion of a mass m , directed or non-isotropically turbulent, generates a mass flux which, per unit surface at time t and location z along the radius, is

$$\vec{F}_m(z, t) = m \int_{\vec{v}} \vec{v} f(\vec{v}, z, t) d^3\vec{v}$$

where $f(\vec{v}, z, t)$ is the distribution function of velocities \vec{v} . The existence of such a mass flux does not mean that the star is, at each z , in hydrodynamical flow: This may be the case (expansion, mass inflow, mass loss) but other situations exist where the mean value of \vec{F}_m is zero over t (e.g. acoustic waves), or over some characteristic length (e.g. convective motions). The mass flux is accompanied by a mechanical energy flux

$$\vec{F}_{me}(z, t) = m \int_{\vec{v}} v^2 \vec{v} f(\vec{v}, z, t) d^3\vec{v}$$

Hence a mass flux over a certain depth range $\{z\}$ in the atmosphere is a necessary condition to have a non-radiative energy transport (We will not consider magnetic energy here.).

But a mass flux is by no means a sufficient condition of existence for a chromosphere. Mass flux can indeed be present in the photosphere, and, strictly speaking, it implies departures from radiative equilibrium and from hydrostatic equilibrium there. But in the photosphere, there is, by definition, no dissipation of mechanical energy. By contrast, in the chromosphere, as soon as characteristic particle velocities become some fraction of the sound velocity, the energy contained in macroscopic motions is converted into microscopic, thermal ones and heating starts. Then, physically, the base of the chromosphere (or of the chromosphere-corona) is the lowest place where this dissipation starts to be effective.

The observables which point out a chromosphere are either direct indicators or indirect ones. *Direct* indicators are spectral features whose origin is in the chromosphere itself; they directly imply a chromosphere, provided a theoretical analysis allows one to attribute them to such a region. As an example, a line core presenting an emission may imply a source function that does not decrease monotonically outwards. It can be a sign of T_e increasing outwards in the atmosphere (cf. Jefferies's talk). In such a case, a correspondence is established between the observable and the location where T_e rises, identified with the chromosphere, if moreover, this rise in T_e is not produced under RE.

Not all direct indicators of chromospheres have been analyzed in full detail; some of those which have not been analyzed are nonetheless said to be heating indicators, although only on analogical grounds at the moment.

Indirect indicators are phenomena observed in the photosphere *or* in the chromosphere, from which one can predict the presence of a chromosphere, without those indirect indicators necessarily being found jointly with direct observed effects. They include all signs of the presence of non-radiative energy sources. Interpretation of these signs leads not to a local T_e , but to the recognition of the presence of mechanical energy, which might dissipate higher up in the atmosphere, or at the location where the sign is formed.

In the case which we will exclusively consider in the following, namely, production of chromospheres from dissipation of mechanical energy, such indirect indicators directly reveal the existence of a mass flux in the star. Examples are oscillatory motions in the solar low chromosphere, astronomical turbulence, solar granulation, etc. . . .

THE BOTTOM OF THE CHROMOSPHERE

The organizers of this conference asked for a discussion on the most valid criterium to decide where the chromosphere indeed begins. Are we in the chromosphere as soon as the temperature gradient dT_e/dh , derived from observations, is positive? Have we enough tools of analysis to non-equivocally attribute a positive dT_e/dh to a pure RE effect or to a dissipation of mechanical energy, or to both? Let us consider different possible situations and their meanings. A first case is that in which $dT_e/dh < 0$, or T_e is decreasing outwards monotonically. This case is met when there is either pure radiative or radiative plus convective energy transport, and when inelastic collisions maintain populations of energy levels.

When, in a continuum j , photoionizations take over from collisions, the effect first shown by Cayrel (1963) to act in the solar H^- continuum produces an increase of T_e under RE. If we ignore the lines, T_e may increase up to some colour temperature T_c , characteristic of the most transparent continuum. Each continuum successively contributes to the increase in T_e (see Feautrier, 1968; Auer and Mihalas, 1969, 1970; Mihalas and Auer, 1970; Gebbie and Thomas, 1971). The location of the layer where T_e starts to rise is both frequency dependent, because the rate of photoionization in each continuum σ_{jc} is, frequency dependent and density dependent through the rate of collisional ionization Γ_{jc} . This dependence evolves from star to star along the spectral sequence with the nature of the main absorber in the transparent layers of the star (H^- in the Sun and F, G, and K stars; HI in hotter stars; HeI ?) and with the gravity, which, combined with T_e , governs the electron density in the star.

But, as radiation is not carried exclusively in the continuum, lines enter to modify the preceding conclusions. To be brief, let us mention the work by Frisch (1966), and Athay (1970), who conclude that the lines they have considered (lines not coupled to the continuum) act as cooling agents in the Sun.

In consequence, even if dT_e/dh is inferred to be negative from observations, in a region where one can show that the density is low enough that $\Gamma_{jc} \ll \kappa_{jc}$ but where the effect of lines is mainly to cool, we are in a practical situation in which we are not able to recognize the starting layer of the Cayrel effect. Suppose now that observations lead to $dT_e/dh = 0$. It may mean that the Cayrel effect is present but exactly balanced by cooling due to lines; or that we have the same, plus a strong cooling due to lines, but with a contribution of heating by mechanical dissipation. If properly analyzed observations lead to $dT_e/dh > 0$, either one has one of the former situations, with continuum influence, plus some lines coupling to the continuum to produce a heating effect stronger than the cooling due to other lines; or the same plus mechanical heating; or mechanical heating alone, if, for instance, T_e is higher than the colour temperature T_c of the most transparent continuum.

My conclusion is that it is impossible, at present, to decide unambiguously what is the proper interpretation of a dT/dh inferred from observations in the low density layers of a stellar atmosphere, without having carefully studied which are the opacity sources and how lines interact with them in governing the temperature run, as well as the mechanical energy sources and where their energy is dissipated. Despite valuable efforts on this purely theoretical problem, a considerable amount of work is still needed to unravel the non-LTE photosphere from the chromospheric regions.

But the Cayrel effect in no case can increase T_e over T_c . If, then, through appropriate observables, one diagnoses a temperature higher than T_e , one can claim, without the detailed analysis of all the above mentioned physical processes, that mechanical heating operates and that one sees the chromosphere. However, at present, direct indicators of a chromosphere cannot by themselves lead to the location of the base of the chromosphere, not even in the Sun.

Considering that in the Sun the question of the bottom of the chromosphere is not settled, and that the best semiempirical models have been obtained from eclipse data and from high resolution disk spectra in the core of strong lines and in UV and IR continua, we will not be able, in stars, both from lack of theoretical analysis and from the lesser quality of observations, to fulfill the program announced to be ideal in this

introduction. Only a survey of observables and an attempt to classify them are possible, and we will make such a survey in the following paragraphs.

SURVEY OF RECENT OBSERVATIONS

Two review papers on observations of stellar chromospheres were presented in 1969 (Feast, Praderie). We will attempt here to gather the recent observations and some of those which were omitted in the previous reviews and will examine successively indicators of mechanical energy dissipation, some selected indicators of mass flux, and after the Sun's example, indicators of horizontal inhomogeneities and of temporal variations in chromospheres. The present survey is restricted to observations in the visible and in the infrared.

INDICATORS OF HEATING

These indicators are mainly line profiles showing excitation-ionization anomalies; UV and IR continua have already been mentioned (Praderie, 1970). The identification of lines as chromospheric indicators proceeds from the theoretical understanding of their formation. The most famous example is that of the solar H and K central reversals (Jefferies and Thomas, 1959). As recalled by Jefferies during this conference, all the so-called collision dominated lines are, in the same way, model dependent and may reflect chromospheric values of T_e and N_e . Emission in some other lines is not as well understood, as the following examples will show.

Excitation anomalies include, first, the extreme case of all lines in emission (examples: Wolf-Rayet stars spectrum, or the solar spectrum below 1800 Å); second, the case where some lines are in emission (examples: He II $\lambda 4686$ in Of stars, MgII and CaII resonance doublets in the Sun and many late type stars); third, the case where absorption lines appear which correspond to an excitation much higher than that existing in the photosphere (examples: He I $\lambda 5876$ or $\lambda 10830$ lines in cool stars).

Ionization anomalies include the presence of lines of highly ionized atoms (coronal lines) and (or) of a continuum emission in the radio wavelengths, emission whose origin is probably in a hot corona.

EXCITATION ANOMALIES

H AND K LINES OF CA II

Observations of the central emission in the resonance doublet of Ca II, which were extensively made by Wilson and Wilson et al (1954, 1957,

1962, 1963, 1964, 1966, 1968) and others, have been pursued actively, not so much to study individual atmospheres, as to take advantage of the presence of this feature to derive other stellar properties to which the emission is correlated. These correlations may lead to a better understanding of the sources of heating of the chromospheres as functions of spectral type (Skumanich, 1972). We first consider here time-independent observations:

- Dependence of H and K emission with bolometric luminosity (Wilson, 1970 — For 65 stars of the same age (F 4 to K 5, main sequence Hyades stars) the mean flux ratio for the emission components of H and K increases from $B - V = 0.45$ to $B - V = 1.25$, and the emission intensity to bolometric luminosity ratio increases by a factor of 2 in the same spectral type range. It is not known if this trend is universal, or if it is age dependent.
- Dependence of H and K emission with age of the star — From Wilson's work (1963), it is known that field main sequence F and G stars, studied at 10 Å/mm dispersion, show no more emission for stars hotter than F 5, and that 10% of the stars of type later than F 5 have an emission in H and K. For F and G main sequence stars in galactic clusters, all stars of type later than F 5 have an emission in H and K. Wilson and Woolley (1970) have studied the Ca II emission at 38 Å/mm in 325 main sequence stars. The emission is found to be intense for stars whose orbit eccentricity is close to one and whose orbit inclination relative to the galactic plane is weak, hence which are the youngest in the sample. It is concluded that Ca II emission is one of the best age indicators available, being the weakest when the star is advancing in age. As a result of this age dependence, H and K emission has been used as a tool to detect faint members in young clusters (Kraft and Greenstein, 1969). Because the majority of the members of the Pleiades (according to proper motion) have K_2 emission twice as strong as Hyades stars of the same type, the assumption was made that such an emission identifies members of the cluster even for stars fainter than $V = 13$. Observations have been successfully conducted at 200 Å/mm for stars later than K 5 in the Pleiades. Prolongation of the main sequence toward faint members allows a determination of the contraction time of the stars in the cluster.
- Ca II emission and polarization. Dyck and Johnson (1969) have shown that the deviation of the mean degree of intrinsic polarization per night relative to the mean degree is anti-correlated to the intensity in K_2 for ten cool giants and supergiants. These observations have been extended to long period irregular variables by

Jennings and Dyck (1971). In those stars, H and K emission occurs only if the polarization degree is weak (0.1%), and it is exclusive with IR emission around 10μ . It is suggested that polarization and IR emission are due to a dust shell, the formation of which prevents a strong heating of the chromospheric gas.

- Ca II emission in binary systems. Popper (1970) mentions that 25 eclipsing systems are known with emission in H and K in the primary or in the secondary component; their types are F to K O. The emission may undergo the eclipse. It is observed in dwarfs as well as in supergiant systems. Carlos and Popper (1971) have found the same effect in a spectroscopic binary, H D 21242, the emission being localized in the spectrum of the secondary (K O IV; the primary being G 5 V). Inversely, the presence of a strong K_2 emission in giants can be used to detect binary systems. Abt, Dukes and Weaver (1969) have studied 12 Cam (KO III) and checked that assumption with success.
- Wilson-Bappu effect. The well-known empirical relationship established by Wilson and Bappu (1957) for G, K and M stars is

$$M_v = 14.94 \log w_o + 27.59$$

where M_v is the visual absolute magnitude, and w_o is the width of the emission, corrected from the instrumental profile. I will not discuss this relationship and its evolutive implications here, except to mention that it has been recently extended to 200 more southern stars (Warner, 1969). The question of calibration in terms of absolute magnitudes has been critically reviewed by Wilson (1970). A possible influence of metal abundance which could perturb the general use of the relationship and was suggested by Pagel and Tomkin (1969) receives objections from Wilson in that article.

Let us recall that not all stars showing H and K emission obey the WB relationship; T Tauri do not (Kuhi, 1965); nor do Cepheids (Kraft, 1960). But the Sun does verify the WB relationship. This is why attempts to explain the luminosity effect on the K_2 emission width have turned first to the physical parameters of the solar chromosphere, where it is formed. Turbulence has not proved to be the key, although it was shown, originally by Jefferies and Thomas (1959), and more recently by Athay and Skumanich (1968) that the emission width, defined by Wilson, is indeed a function of the Doppler width. Recently, studies of high resolution spectrograms of the Sun have been performed, with the aim of recognizing the contribution of discrete chromospheric elements in the formation and position of the K_2 peaks of Ca II, by Pasachoff (1970, 1971) and by Bappu and Sivarawan (1971). By a careful study of a series

of K profiles and of K_{232} spectroheliograms in the quiet Sun, Bappu and Sivaraman have derived the distribution of the K_2 peak to peak distance on the solar surface. This width can of course be measured on spectra only when both K_{2R} and K_{2V} exist as bright features (about 95% of the situations). In that case, the WB relationship is satisfied. From a study of intensity fluctuations in K_{2V} and K_{2R} along the slit, the authors identify the emitting regions for which the WB relationship is valid with the bright fine mottles. On the other hand, it is known that the K_2 width decreases over plages (Smith, 1960), and at the super-granulation boundaries, where magnetic fields of the order of 100 gauss are present. Those two results suggest: (1) that in stars where the K_2 width obeys the WB relationship, an inhomogeneous structure like the solar mottles exists, and (2) that a deviation from WB relationship will occur in particular in stars with a magnetic activity, and will also tend to be associated with a light variation. According to Bappu and Sivaraman, the rotation of the star is a decisive parameter in modulating the rate of plages on the visible disk. At the present stage, and in spite of its interest, it is clear that this interpretation of the WB effect is somehow incomplete, in the sense that it does not offer a reason for the variation of the properties of the fine mottles with luminosity in such a way that w_0 is kept proportional to visual luminosity $L_v^{1/6}$.

An example of the above picture seems to exist; γ Boo (A 7 III) is a star with a high rotational velocity ($v \sin(i)=135$ km/s); it shows short time scale variations in the K line core. That is, it exhibits variable asymmetry, and despite the high $v \sin(i)$, the temporary occurrence of an emission (Le Contel et al., 1970). The K emission width is smaller than that expected from the WB relation, which fits Bappu and Sivaraman's suggestion if emission comes only from plages; the star is also variable in light; one of the proposed interpretations for these phenomena is that the star's surface is perturbed by plages. An extension of this scheme of interpretation to deviations from the WB relationship for Cepheids or T Tauri seems hazardous at the moment.

IR TRIPLET OF Ca II — The infrared lines of Ca II near to 8498 Å show no central emission in the quiet Sun. An emission core is seen over plages, the most intense being in the otherwise weakest line of the triplet, as was beautifully described by Linsky during this conference. In long period variables, like R Leo (M 8 e), Ca II triplet occurs in emission (Kraft 1957); in T Tau stars it occurs also.

BALMER LINES OF HYDROGEN — Because their source functions have source and sink terms dominated by photoionizations in solar type stars, these lines are comparatively insensitive to the local physical characteristics of the atmosphere, and depend mainly on the radiation field in the

various continua (Thomas, 1957). The influence of a decrease of gravity is to enhance the photoelectric character of the source function. As suggested by Mihalas, the character of the Balmer lines source function changes in hot stars. Therefore, the observed emission of H α in hot supergiants, if not due to a geometrical effect, could be a sign, not of a chromosphere, *as previously defined*, but of a non-LTE photosphere. But H α in emission is not found only in hot stars. It appears in d M e stars, often simultaneously with K emission; in symbiotic stars where emission lines are superimposed on an M type spectrum; in flare stars; in T Tau stars, etc. (Bidelman, 1954; Herbig, 1960).

Wilson (1956) reported emission in He, observed on 10 A/mm spectra of K and M type stars. Emission is first observed in K stars, and is well developed in M giants, but not in the supergiants. Excitation of the 7th level of Hydrogen by the Ca II H line does not seem likely, as H ϵ lies too far in the wing of the H line ($\Delta\lambda = 1.58 \text{ \AA}$). Ly η could do the same, but until now it has not been observed in those stars. One wonders why only this single Balmer line (H), would be in emission through such an excitation process.

Other Balmer lines can be in emission in special groups of late type stars (symbiotic stars, Mira variables). A recent observation reports H γ and H δ in emission in α Ceti at phases close to the maximum of light (Odell et al., 1970).

PASCHEN LINES OF HYDROGEN — Pa has been predicted to be in emission in O stars under radiative equilibrium (Mihalas and Auer, 1970), but observational difficulties at that wavelength (1.8751μ) have until now prevented a check of this prediction, or finding other stars where this emission could occur. But P β (1.2818μ) and P γ (1.0938μ) have been observed in emission: P β in α Ceti (Kovar et al., 1971), and P γ in γ Cas (BO IV e), which is not a shell star (Meisel, 1971).

No equivalence of emission cores in Paschen or Balmer lines exists when observed over the disk in the Sun.

HELIUM I LINE — The triplet series lines $\lambda 10830$ and $\lambda 5876$, in absorption or in emission, correspond to a high excitation, and are not of photospheric origin in late type stars. $\lambda 10830$ ($^3S - ^3P^o$) has been discovered in emission in P Cyg and in carbon Wolf-Rayet stars (Miller, 1954), then in emission in all Wolf-Rayet stars (Kuhi, 1966). Vaughan and Zirin (1968) have searched for this line in 86 stars at 8.4 A/mm and found it in absorption in normal G and K stars, and in emission in five stars, where the profile is of the P Cyg type. Meisel (1971) observed it in emission in γ Cas. The presence of $\lambda 5876$ ($^3P^o - ^3D$) is attributed to hot chromospheric layers in late-type stars. Wilson and Aly (1956) detected it

in G and K stars, the warmer being of type G 5 V (κ Ceti). Feast (1970b) found this same line in φ Dor (F 7 V), a star which otherwise has also an intense emission in H and K. Fosbury and Pasachoff reported more observations during this conference.

In the Sun, besides the flash spectrum, $\lambda 5876$ (also called D3) is observed in absorption only above active regions; $\lambda 10830$ is seen in absorption over selected regions of the disk (network cells, plages and filaments) (e.g. Zirin and Howard, 1966). Both lines can be observed in emission only in bright flares. They are assumed to be formed in the strongly non-homogeneous chromosphere, namely in the hot regions, above 2000 km from the limb.

Coming now to a quite different class of objects, it has been argued by several authors (Nariai, 1969; Wickramasinghe and Strittmatter, 1970; Böhm and Cassinelli, 1971), that helium stars and white dwarfs could have a chromosphere-corona, because, according to the mixing length theory, their convection zone is predicted to be important (effect of increased He abundance or of density). Nariai gave ν Sgr as a good candidate. Observations performed on the helium star G 61-29 show broad He I emission lines, among which $\lambda 3889$ has a central reversal (Burbidge and Strittmatter, 1971). No detailed interpretation of any of these He I lines in helium stars has yet been worked out, but the He I and He II spectrum in O stars is the object of an important study by Auer and Mihalas (1972). For many other lines, which might be related to chromospheres, no detailed analysis is yet available. We will only briefly mention them now.

OTHER EMISSION LINES

- The K I resonance doublet seems to appear definitively in emission in a small number of very peculiar stars such as the long period variable χ Cyg, the peculiar supergiant VY C Ma. A single reversal is also seen in the core of this line when observed in sunspots (Maltby and Engvold, 1970).
- The O I infrared line at $\lambda 8446$, observed by Wallerstein (1971) in stars showing an IR excess occurs in emission when Ca II $\lambda 3933$ is broad, while it shows no emission when Ca II is sharp and in emission.
- Fe II also builds an emission spectrum in many late type stars as well as in some early types and in symbiotic stars (see e.g., Bidelman, 1954; Herbig, 1960). Can one say that their origin is chromospheric, or do they show an increase in excitation in a rather cool (relative to a chromosphere) circumstellar shell? Weymann (1962) attributes those Fe II lines observed in α Ori around 3100 Å to a chromo-

sphere, although in that star the Fe I excitation temperature is very low, and Fe I lines are formed in a shell. Those lines are often simultaneously present with an excess of IR in the 2-10 μ range. Geisel (1970) gives a list of 35 stars, mainly hot (Be - P Cyg, Ae, Fe, Ge, and some others) where the IR excess has been predicted, and found, from the physical relationship between Fe II and [Fe III] emission and the IR excess. Such a correlation, if extended, and the already quoted exclusivity effect between Ca II K emission and polarization plus infrared excess put in full light the problem of the mutual relationship of chromospheres and dust shells around Be stars as well as around cool stars.

All the excitation anomaly indications reviewed here are lines. Moreover, all the corresponding observations concern the integrated disk of the star. In the perspective of having the Sun as a running example, we must stress that the first modern models of the solar chromosphere have been derived from the analysis of eclipse data (emission gradients in Paschen and Balmer continuum, lines of metals, Balmer lines . . . see Thomas and Athay, 1961). A limited number of eclipsing systems consist of a main sequence B star and a K or M supergiant whose chromosphere is illuminated by the B star light during the eclipse. Those stars contain more information on chromospheric layers of the K or M component than any other observed only in the disk. Their prototype is ζ Aur. A review of observations and interpretations was given by Groth (1970); they will not be mentioned further here, in spite of their major interest in attacking the chromosphere problem in stars.

Besides the eclipsing systems of the ζ Aur type, several groups of stars deserve special attention relative to the observations of chromospheres. Some were incidentally mentioned: Mira variables, Wolf Rayet stars, T Tauri, helium stars, symbiotic stars. We shall add flare stars but it is not possible here to give a meaningful account of them. A recent paper on chromospheres in flare stars is that of Gershberg (1970), and a review has been given by Lovell (1971).

IONIZATION ANOMALIES

Observations of the radio continuum have been performed, without success, on α Ori M 2 I ab) at $\lambda = 1.9$ cm by Kellermann and Pauliny-Toth (1966), and with success on α Ori and π Aur (M 3 II) at $\lambda = 2.85$ cm by Seaquist (1967) and on α Sco at $\lambda = 11.1$ cm by Wade and Hjellming (1971) and Hjellming and Wade (1971). In this last case, the radioflux at 3.7 cm happens to be higher or smaller than the 11.1 cm flux, showing that the source is variable both in intensity and spectral index; the source seems to be associated with Antares B (B 3 V) rather than with Antares A (M 2 I b).

Coronal type lines have been observed in the spectrum of novae. The identified lines, allowed or forbidden, belong to highly ionized atoms. A bibliography can be found in the C.N.R.S. International Colloquium on novae, supernovae, novoides (1965). Recent work due to Andrillat and Houziaux (1970a, 1970b) identifies coronal lines in the near infrared region of Nova Del 1967: lines of [Fe X], [Fe XI], [A XI], [Ni XV].

Solar chromospheric temperatures have been derived from the observations of mm and cm radiation jointly with eclipse data to infer densities (e.g. Dubov, 1971). As to the coronal lines, their ionization and excitation mechanisms are fairly well understood in the Sun. But very few attempts have been made to extend the solar corona type of analysis to novae.

One of the lines having been used to characterize the properties of the transition region between the solar chromosphere and corona is the O VI doublet at 1031.9 - 1037.6 Å. I don't know of any observation of this line in stars, but other O VI lines are well known in Wolf Rayet stars, and have been reported in planetary nebulae central stars and in stars which are not central (Sanduleak, 1971).

INDICATORS OF MASS FLUX

To review all indicators of mass flux in photospheres is beyond the scope of this talk, although it would be most valuable to do so, and to examine simultaneously why some velocity fields become turbulent and others do not, and why some of them evolve until their energy is converted back to the thermal pool of the atmosphere by heating.

Let us focus our attention here only on those mass flux indicators which pertain to the chromospheric layers themselves, because these indicators are lines formed in the chromosphere. The whole question of mass *loss*, namely of net systematic escape of matter from the star, will be set aside.

ANOMALOUS LINE-WIDTHS

A good example is that of H α in the solar chromosphere. This line alone cannot lead to an inference of T_e in chromospheric layers. But suppose we know T_e (h). To interpret the halfwidth of this line, as well as of others, a statistical broadening of the Doppler type must be added to the thermal one. This additional broadening is attributed to microturbulence.

In stars, assuming that the core of H α is formed in the same layers as the emission peaks of the H and K lines, Kraft et al. (1964) studied the width of H α called H_0 . They found a correlation between H_0 and the absolute magnitude in the U band pass. This work has been repeated by Lo Presto (1971) with improved observational facilities. He observed

about ten stars with the solar tower at Kitt Peak, and obtained a better relation than Kraft's between H_α and M_v . This result extends in fact to H_α the Wilson-Bappu relationship, without, nevertheless, reinforcing an interpretation of this relationship in terms of solely a turbulence effect. Further work is in progress on late-type stars of all luminosity classes (Fosbury, 1971).

No exceptions have been reported (to my knowledge) to the empirical relationship between H_α and M_v and so there is no counterpart on H_q to T Tau or Cepheids disobeying the WB relationship.

According to Vaughan and Zirin (1968), the He I $\lambda 10830$ line seems also to show a broader profile than photospheric lines in stars where it has been observed.

The same is true (enhanced line-width, from which astronomical turbulence is invoked) for Wolf-Rayet stars emission lines, certain of which show a P Cyg profile, and hence reflect that the emitting region experiences mass ejection.

ASYMMETRICAL LINES

Both Ca II and Mg II resonance doublets are strongly asymmetric in the quiet Sun (e.g. Pasachoff, 1970; Bappu and Sivaraman, 1971; Lemaire, 1971). For Ca II, a statistical analysis has been performed by Bappu and Sivaraman on the occurrence of different patterns for the relative K_{2V} and K_{2R} intensities: $I_{K_{2V}}$ is bigger than $I_{K_{2R}}$ in 45% of the profiles; they are equal in 4.7%; $I_{K_{2V}}$ is smaller than $I_{K_{2R}}$ in 25%; $I_{K_{2R}} = 0$ in 22.3%; $I_{K_{2V}} = 0$ in 0.7% of the cases.

In stars, the profiles of Ca II K line obtained by Liller (1968) or by Vaughan and Skumanich (1970), even if they show only one central emission core, are very far from being symmetric. The core of H_α is also often asymmetric in late type stars (see Kraft et al., 1964; Weymann, 1962). Some of the He I $\lambda 10830$ profiles observed by Vaughan and Zirin in hot stars exhibit a P Cyg type profile. The chromospheric layers are then associated with directed velocity fields indicating mass transport towards the interstellar medium.

INDICATORS OF HORIZONTAL INHOMOGENEITIES AND OF ACTIVITY

In the Sun, Doppler shifts and intensity fluctuations along the slit in lines allow study of both the propagation of waves and the solar fine structure in the upper photosphere and low chromosphere. On the other hand, on spectroheliograms and filtergrams, one sees the coarse network, coarse mottles and fine mottles, as well as spots, filaments and other features, and inhomogeneities prove to extend high up in the chromosphere. In stars, no such observations can be performed, except possibly in eclipsing

systems of the ζ Aur type. We will then restrict ourselves here to indicators of temporal variations in stellar chromospheric spectra, ignoring spacial inhomogeneities.

Variations have been observed in H and K lines and for stars where these lines happen to show central emission. Several other chromospheric lines undergo variations also.

In H and K, these variations affect the intensity of the emission peaks and the shape of the profile. Let us consider first late-type stars. Griffin (1963) and Deutsch (1967) first reported such variations in α Boo and other cool giants. Variations in the K emission can be occasional (e.g. Kandel, 1966, in the dwarf HD 119850; Boesgaard, 1969, variations in the MS star 4 Ori). Although they have been searched for, to the best of my knowledge, no cyclic variations in the K line flux have yet been reported (Wilson, 1968; Liller, 1968). This might only reflect the lack of long enough time sequences of observations.

If these variations are associated with changes in the physical properties of the emitting atmosphere (occurrence of plagues, for instance), one wonders if this activity is correlated with a general brightness variation of the star. Such photometric variations have been searched in the UBV filters by Blanco and Catalano (1970), on HD 119850 (d M 2.5 e), α Boo (K 1 III) and α Tau (K 5 III). No clear variations can be detected. Similar observations were made by Krzeminski (1969) on a sample of d Me and d M stars. Light variations exist in some d M e stars, showing that activity is a continuous process; but none are present in d M. The extreme example of stars showing activity in light as well as in chromospheric lines (H α core, Ca II) is that of flare stars, also classified as UV Ceti variables.

Among variables with chromospheric characteristics, Mira stars also prove to be variable in their emission lines; e.g., variation in H γ , H δ reported by Odell et al., (1970), variation in P β reported by Kovar et al., (1971).

Toward hotter stars, the already mentioned A 7 III star, γ Boo, shows a quasi-periodic velocity field from radial velocity measurements at mid-intensity in the K line, and a variable K line reversal within time intervals of 2 hours (Le Contel et al., 1970). Due to lack of observations, no period has been recognized for the K line core variation; hence, it has not been related to the light variation which the star experiences with a period of 0.29 d. The light amplitude is variable, and phases of calm with no variations at all do exist.

In *Of stars*, which have not been considered in detail in this paper, variations in strengths of the emission lines N III λ 4034, 4640, 4641 and He II 4686 have been observed by Brucato (1971) with a time scale of the order of ten minutes. A typical *Of stars*, ζ Pup, is also one of the stars which ejects mass at the highest known velocity (Carruthers, 1968).

The *HeI* line $\lambda 10830$ experiences variation, as in the Sun (Vaughan and Zirin, 1968), in several late type stars.

A puzzling case appears to be that of the star *R Cr B*, whose chromospheric properties have been pointed out by Feast (1970), after Payne-Gaposhkin (1963) and others. The H and K cores, D lines of Na I and sharp Sc II, Ti II, Sr II, and Fe II lines appear in emission when the star (F 7 carbon supergiant and irregular variable) goes through the minimum of light. That phase has been suggested to coincide with the ejection of condensed graphite which obscures the star. If this is the case, it seems difficult to reconcile the presence of this carbon black cloud with that of a chromosphere, namely a heated layer, because to have carbon change phase, one most likely requires heat absorption instead of dissipation. On the other hand, during phases of maximum light, and over one year, *R Cr B* has been variable in the infrared continuum (Forrest et al., 1971) at 3.5μ , while at 11.1μ it was quasi-stable. The variation amounts to 1.5 mag, which means that the circumstellar carbon grains have been heated, whatever the form of energy input. We may assent to a possible alternation between absorption of heat to produce grains, and heating of those grains.

CONCLUSION

Obviously, an enormous gap exists between observations as they stand, on the one hand, and their interpretation in terms of the general structure of a stellar atmosphere, on the other hand.

There is no such thing as an available grid of stellar chromospheric models (although stellar coronas have been quantitatively predicted). One has to realize, case by case, for each interesting star, that the observational information is scarce enough so that one has difficulties applying a solar analogical method, such as described by Avrett, to analyze them. Attempts were made by Kandel (1967) and by Simon (1970) to produce chromospheric models for d M stars, in one case, and for Arcturus (*a Boo*), in the other.

At the moment, we have not fulfilled the scheme for analysis which the introduction claimed to be legitimate in looking at stellar chromospheric indicators. This may mean that we have not given the *useful* definition of a chromosphere required at the beginning of the conference. We have been able to classify many of these indicators by referring them to heating or to mass flux. But we have met at least three important problems on which we have had to be vague. One is diagnostical, and has been outlined by Jefferies. Are all emission lines a signature for a chromosphere? The second is structural. How could we specify the base of the chromosphere at all, and how do we do this when a circumstellar shell is related to it, especially in stars where the shell seems to be very close to the photospheric layers? The third question relates to the physics

of velocity fields. Do all motions detected in photospheres become turbulent and are they a result of atmospheric heating? If not, what are they like and what causes them?

A way to progress is surely to call for more observations, but for more systematic ones, in the sense that we want them to be led as closely as possible by a physical question to answer. The most immediate step to take would be to collect, from a limited number of objects, information from all spectral regions, lines and continua, to be able to construct reliable spherically symmetrical models of $T_e(h)$, those models being obtained using the static energy balance equation, taking into account line effects, and treating the mechanical energy input as a free parameter, if no better treatment is possible. A simultaneous effort should be pursued to answer the precedingly quoted questions, whose answers will influence the construction of a model.

This paper was prepared partly when I was in JILA, as a Visiting Fellow (1970-1971); I have benefitted from numerous clarifying discussions there, as well as in France, and I acknowledge especially the continuous interest of Ph. Delache, J. — C. Pecker, R. Steinitz, and R.N. Thomas. I am also indebted to all colleagues who sent me preprints of their current work before the Goddard Conference.

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EVIDENCE FOR STELLAR CHROMOSPHERES PRESENTED BY ULTRAVIOLET OBSERVATIONS OF THE SUN AND STARS

Lowell Doherty

*Space Astronomy Laboratory, Washburn Observatory
University of Wisconsin*

I would like to describe observations of emission lines in stellar sources, in the ultraviolet region of the spectrum not accessible to ground observation. As we have heard, the interpretation of emission lines may involve both geometrical and temperature effects, so that the occurrence of emission lines does not constitute *prima facie* evidence for chromospheres. On the other hand, we have not yet, at this conference, formulated a definition of a chromosphere that excludes any particular category of stellar emission-line objects.

In principle, information on chromospheric structure is also contained in the continuum. However, the measurement of accurate spectral energy distributions depends on the very difficult process of ultraviolet photometric calibration. This work is continuing both at Goddard and the University of Wisconsin. I will not discuss continuum observations here. Wilson and Boksenberg (1969) have extensively reviewed instrumentation and results in ultraviolet astronomy up to 1969. The most recent results will be discussed in a forthcoming review article by Bless and Code (1972).

Observations of ultraviolet emission lines are as yet confined to a few stars, and I will try to describe most of these observations briefly, with emphasis on work done since Wilson and Boksenberg (1969). Let us begin with the stars of earliest spectral type. The spectra of Wolf-Rayet stars are sprinkled with the resonance lines of C, N, and Si, excited lines of these elements and of He II. Figure II-1 shows OAO photoelectric scans of two Wolf-Rayet stars. The short-wavelength segments of these scans ($\lambda < 1800\text{\AA}$) have a resolution of about 12 \AA , while the long-wavelength segments, made with a different spectrometer, have a resolution of about 25 \AA . Even at the low resolution of these scans, P Cyg profiles are evident in a number of lines, especially the resonance doublets N V $\lambda 1240$ and C IV $\lambda 1550$. In HD 50896 (WN5), $\lambda 1496$ and $\lambda 1719$ of N V and $\lambda 1640$ of He II are strong, as are other longer-wavelength lines of N and He. In γ Vel (WC7) the C spectrum is well developed. γ Vel has also been observed at 10 \AA resolution (Štecher 1970) and photographically at higher resolution (Wilson and Boksenberg 1969). L. Smith (1972) has interpreted the strengths of ultraviolet C, N, and O lines in HD 50896 to

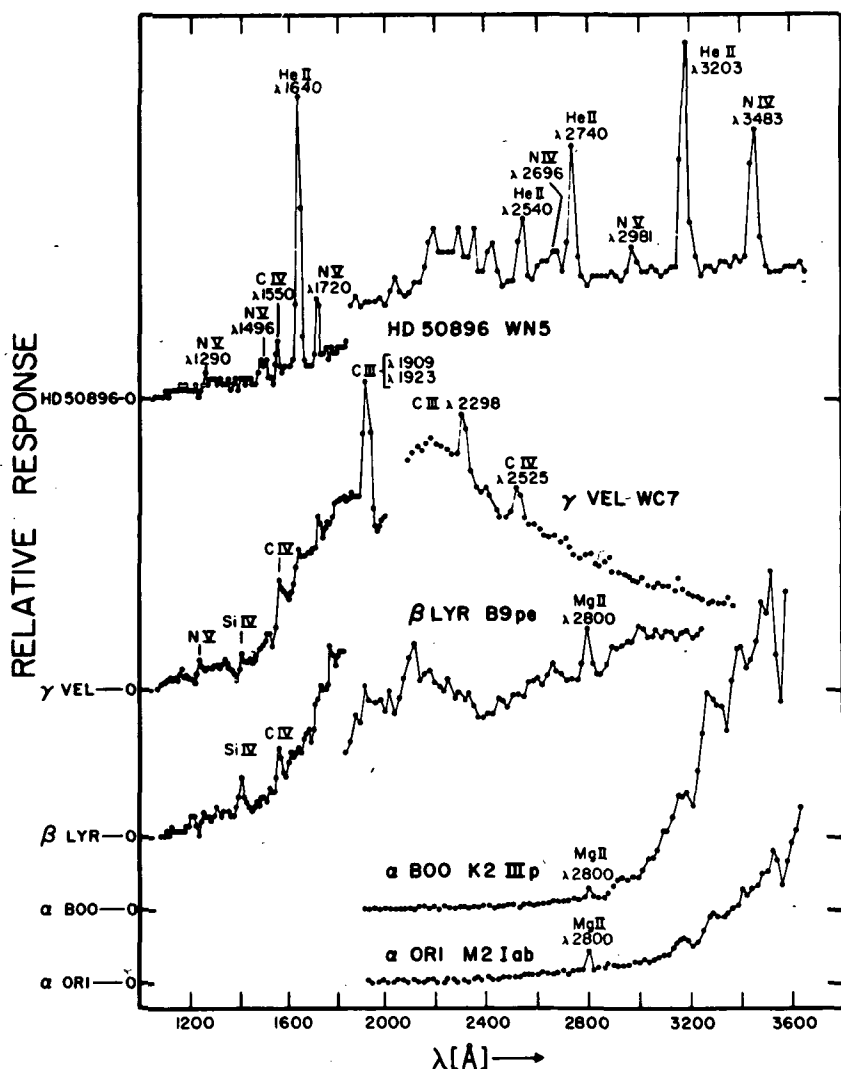


Figure II-1 OAO scans of selected stars. Short-wavelength segments have a resolution of approximately 12 Å, and the long-wavelength segments 25 Å.

mean that selective excitation processes are unimportant, with the implication that differences between WN and WC spectra reflect real abundance differences.

Among O and B stars, emission has been observed in 6 Orion stars of spectral type O9 to B2 and luminosity class I to III, and in ζ Pup (05f) and ξ Per (07). Analysis of 2Å resolution photographic spectra of 5 of the Orion stars (Morton, Jenkins and Bohlin 1968) established that

expansion velocities of some 1500 km/sec exist in the envelopes of these stars, and that there is a velocity gradient for the ultraviolet lines. The highest velocities were obtained from the absorption components of the P Cyg profiles of resonance lines of Si III, Si IV, C IV, and N V. For $\lambda 1175$ of C III, velocities were between 500 and 1000 km/sec, substantially lower than for the resonance lines. Since $\lambda 1175$ arises from a 6 eV excited level of the resonance triplet and is presumably formed closer to the stellar surface, the velocity of expansion must increase outward. Later A. Smith (1970) and Carruthers (1971), with resolution close to 1 Å, obtained $\lambda 1175$ velocities near 1600 km/sec in the two very hot stars ζ Pup and ξ Per. However, as Carruthers points out, there is the possibility of blending of the C III lines with N IV $\lambda 1169$.

A. Smith (1970) recorded the spectrum of ζ Pup nearly to the Lyman limit and found the resonance lines of O VI and S VI, which had previously been observed only in the solar spectrum. S VI $\lambda 933$ has a velocity of 1380 km/sec, while O VI $\lambda 1030$ and the $\lambda 990$ resonance line of N III have velocities close to 1800 km/sec, which is typical of the resonance lines at longer wavelengths in ζ Pup. The more recent observations also suggest a greater range of velocities. Carruthers (1971) found 2650 km/sec for N V $\lambda 1240$ in ξ Per, while A. Smith (1970) determined the very low value of 150 km/sec for the excited $\lambda 1340$ line of O IV.

A number of emission lines in ζ Pup, e.g. N V $\lambda 1240$, Si IV $\lambda 1400$ and C IV $\lambda 1550$, are sufficiently strong to be detected in OAO scans. The Si IV and C IV lines have also been seen in ζ Ori (09.5 Ib) and κ Ori (B0.5 Ia), and Si IV $\lambda 1400$ in the 4th magnitude 09.5 supergiant α Cam.

Emission lines have not been found in B dwarfs. For the bright Be star γ Cas, Bohlin (1970) identified the C IV $\lambda 1550$ line as P Cyg type, but absorption features of other resonance lines such as Si III $\lambda 1206$ and Si IV $\lambda 1400$ have their expected wavelengths and are labelled photospheric. Between the excited N IV $\lambda 1718$ line and 2100 Å the spectrum of γ Cas at 2 Å resolution is rather featureless. β Lyrae (B9 pe) shows an emission spectrum which probably arises in a large cloud surrounding the component stars (Houck 1972). A sample OAO scan is shown in Figure 1. In addition to some of the far ultraviolet resonance lines we have mentioned, Mg II $\lambda 2800$ emission is also apparent in γ Lyrae.

Although of less interest, perhaps, for the problem of stellar chromospheres, ultraviolet observations exist for Nova Serpentis 1970 (Code 1972). OAO scans of the $\lambda > 2000$ Å region indicate a changing complex spectrum whose features cannot be easily identified at low resolution.

Among normal stars of later type, the sun, if located a few parsecs away, and viewed with spectral resolution comparable to that used in present

stellar rocket experiments, could be recognized as a star with a chromosphere. Low flux levels would make such observations difficult, however. Shortward of Mg II $\lambda 2800$, the ultraviolet emission spectrum of the Sun does not appear until Si II $\lambda 1810$, and C IV $\lambda 1550$ is the first indication of fairly high temperatures. Observation of the corona would be limited to O VI $\lambda 1030$, since interstellar hydrogen would obliterate the spectrum below the Lyman limit. The solar Lyman lines would also be strongly absorbed.

OAo scans are available for a number of bright stars of spectral type G and later. For such cool stars, data can be obtained only with the long wavelength spectrometer, and in most cases the scans are useful only for $\lambda > 2500$ Å approximately. Figure 1 includes scans of α Boo (K2 III) and α Ori (M2 Iab), which show how rapidly the flux decreases toward shorter wavelengths. Mg II $\lambda 2800$ is clearly in emission in these stars. No features, either in absorption or emission, have been identified for $\lambda < 2800$ Å in OAO scans of these or other K and M stars. Even where counting rates are relatively large, only gross features of the spectrum are apparent at 25 Å resolution. Figure II-2 shows part of an OAO scan of α Cen (G2 V). One OAO (reduced) count equals 64 photomultiplier events. For comparison, the solar spectrum has been smeared to a resolution of 20 Å and normalized to the stellar scan at 2900 Å. The major features of this spectrum are Mg I $\lambda 2852$, Mg II $\lambda 2800$, and the group of Fe II lines near $\lambda 2740$. There is no indication of solar Mg II emission at this resolution. The OAO spectrometer is stepped at intervals of 20 Å, and, as Figure II-2 shows, it would be difficult to interpolate accurately between the discrete data points without the aid of the known solar spectrum. Moreover, scanner wavelengths are normally known only to about ± 10 Å. Thus OAO scans of late-type stars must be interpreted with caution.

Figure II-3 shows the changing character of the spectrum with later spectral type for the region $\lambda > 2800$ Å. Ordinate scales are different for each of the four stars. The location of prominent features of the solar spectrum are marked here for comparison. The scans at least appear to form a fairly smooth sequence with differences attributable to differences in excitation. One noteworthy feature of α Ori is the bump near 3180 Å which is due, presumably, to Fe II emission which Weymann discussed a number of years ago (1962). It is not known where these lines are formed. Profiles of one group of lines are similar to solar Ca II K, but complex velocity fields make the interpretation of these lines difficult. I believe Ann Boesgaard will report on some recent observations of these lines later today. I would like to point out that OAO scans of α Ori set upper limits to the flux in several Fe II multiplets whose upper levels are common to the multiplets that produce the near ultraviolet emission (Doherty 1972).

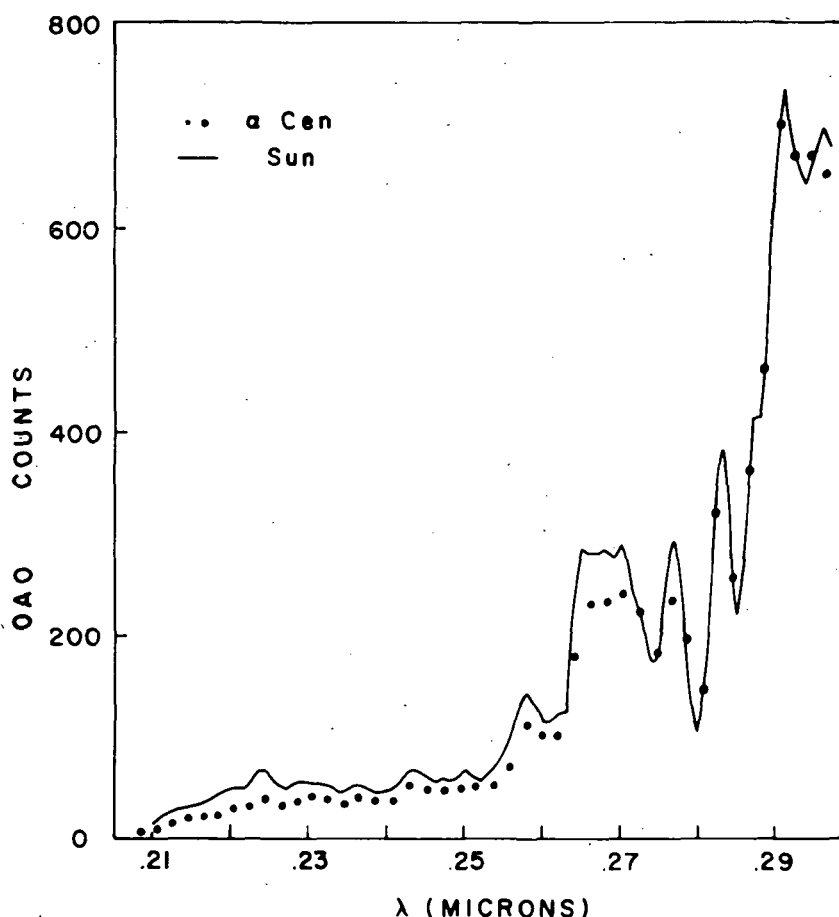


Figure II-2 OAO scan of α Cen compared with the solar spectrum smeared to a resolution of 20 \AA and normalized to the scan at 2900 \AA .

For all very cool, bright stars Mg II emission is clearly seen in OAO scans. Figure II-4 illustrates the 2800 \AA region in several class III giants. Dots indicate OAO (reduced) counts measured at discrete intervals of 20 \AA . Approximate sky background has been subtracted. Exact wavelength registration cannot be determined, but, 2800 \AA does fall between the 5th and 6th channels, as counted from the left. Figure II-5 shows the Mg II region for supergiants. Only the class I stars definitely show emission here. Although Mg II emission fluxes can be determined only approximately from the OAO scans, there is evidence from stars with the strongest emission that the ratio of Mg II to Ca II K emission flux does not differ greatly from star to star. Figure II-6 compares estimated Mg II

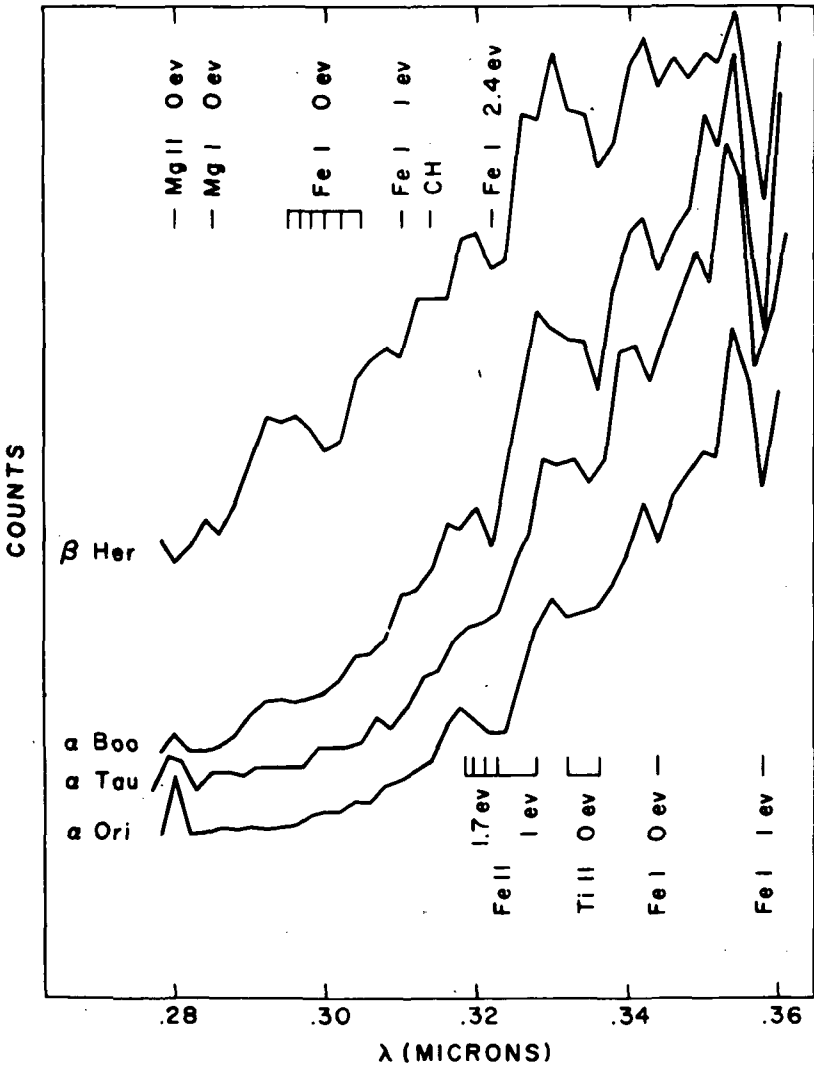


Figure II-3 Changes in ultraviolet spectral features with different spectral type at approximately 25 Å resolution. Ordinate scales are arbitrary. Principal features of the solar spectrum are indicated. β Her, G8 III; α Boo, K2 III; α Tau, K5 III; α Ori, M2 Iab.

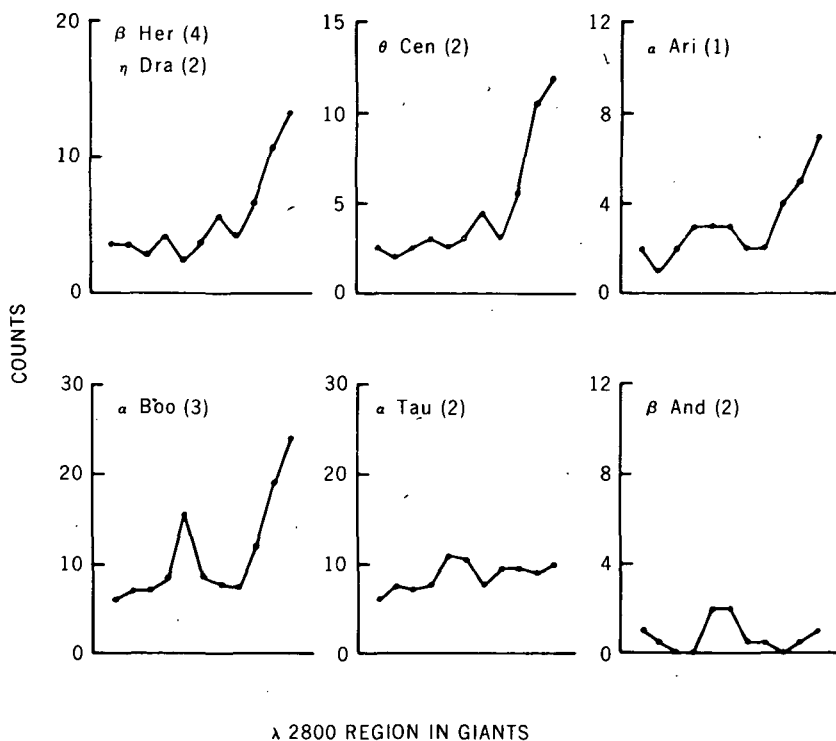
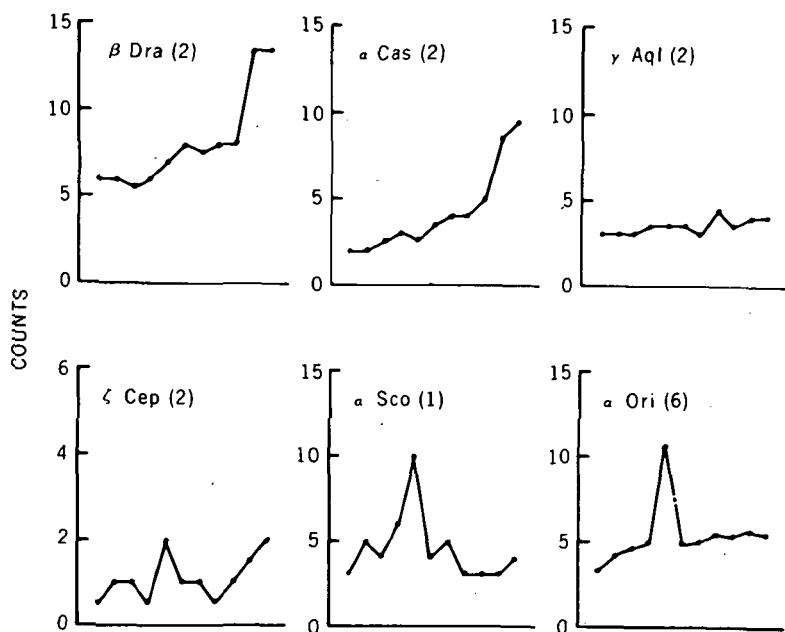


Figure II-4 Spectra of selected G-M giant stars in the 2800 Å region. These averaged OAO scan segments cover 220 Å, with the position of $\lambda 2800$ falling between the 5th and 6th channel as counted from the left.

emission. counts for 8 stars with IW, a measure of the Ca II K emission flux observed at the earth (Doherty 1972).

Vertical bars indicate the limiting values for the Mg II counts that must be assigned as a result of the uncertainty in the strength of the underlying absorption feature. These stars are giants and supergiants of spectral type K2-M2. Within the errors of measurement it is possible that the ratio Mg II/Ca II K is the same for all of these stars. The solar symbol shows the position the Sun would occupy if its visual magnitude were zero. The method of calculating IW does not attempt to subtract the underlying absorption profile of the K line. This does not affect the stellar values appreciably, but the solar value of IW in Figure II-6 represents the total flux emitted in the wavelength band that includes the K emission core and not the net emission. Thus the significance of the approximate agreement between the ratio for the Sun and stars with strong K emission is not immediately apparent.



λ 2800 REGION IN SUPERGIANTS

Figure II-5 Spectra of selected G-M supergiant stars in the 2800 Å region.

Recently, Kondo, Modisette and Giuli (1971) have obtained high-resolution ($1/2$ Å) photoelectric scans of the 2800 Å region in 5 stars covering a wide range of spectral type. The observations were made from a balloon. They find that α Ori has doubly-reversed Mg II cores, qualitatively similar to the profiles of the solar lines. The only other cool star for which Mg II has been observed with better than OAO resolution is Arcturus. At a resolution of 7 Å Mg II appears as a single emission line in this star (Kondo 1972). Arcturus has also been observed in the far ultraviolet by Moos and Rottman (1971) who report the measurement of emission in Lyman α and a line which is probably $\lambda 1304$ of OI.

It is exciting to consider the prospect of having further, more detailed observations of the ultraviolet spectra of stars that we expect to have chromospheres similar to the Sun's. Such observations will, however, be relatively difficult and costly, due to the very low fluxes that must be measured. If we look at the characteristics of the rocket spectrographs (both photographic and electronographic) that have been used to obtain 1

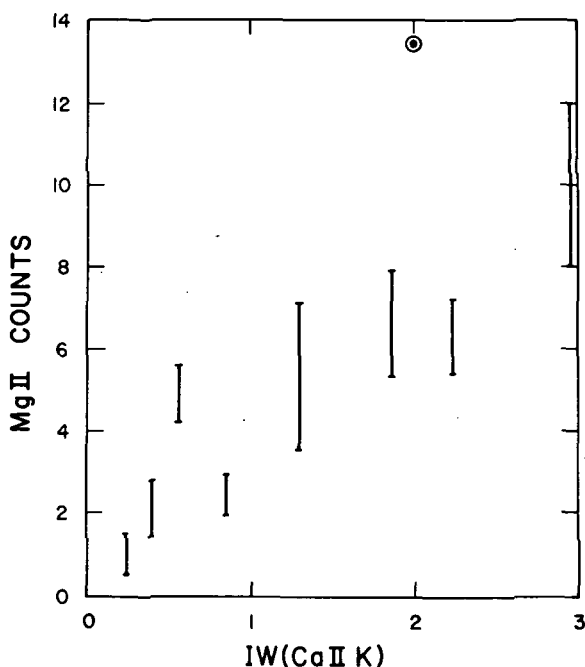


Figure II-6 Mg II $\lambda 2800$ emission (OAO reduced counts) vs. IW, a measure of Ca II K emission flux at the Earth. The Sun is shown as it would appear if it were a V=0 star measured in the same way.

A resolution spectra of O and B stars, these instruments have, on the average, a product of collecting area times exposure equal to roughly $1500 \text{ cm}^2 \text{ sec}$. To obtain the same kind of data for cooler stars of the same visual magnitude, the aperture or the observing time must be larger. In the far ultraviolet, the increase can be enormous. Figure II-7 is a color-color diagram obtained from OAO wide-band filter observations at 1700 \AA . Relative to the visual, the 1700 \AA flux of stars varies by a factor of almost 10^4 from type O to the coolest stars shown, which have slightly earlier spectral types than the Sun. Increases in collecting area and exposure of this magnitude cannot be accommodated in rocket experiments. Thus different techniques must be considered. For example, completely photoelectronic recording can increase the instrumental sensitivity. At present, however, the gain is fully realized only by observation of one spectral band in one object (with one photometer). Continuing development of electronic image intensification and recording systems promises eventually to help this problem by making possible essentially

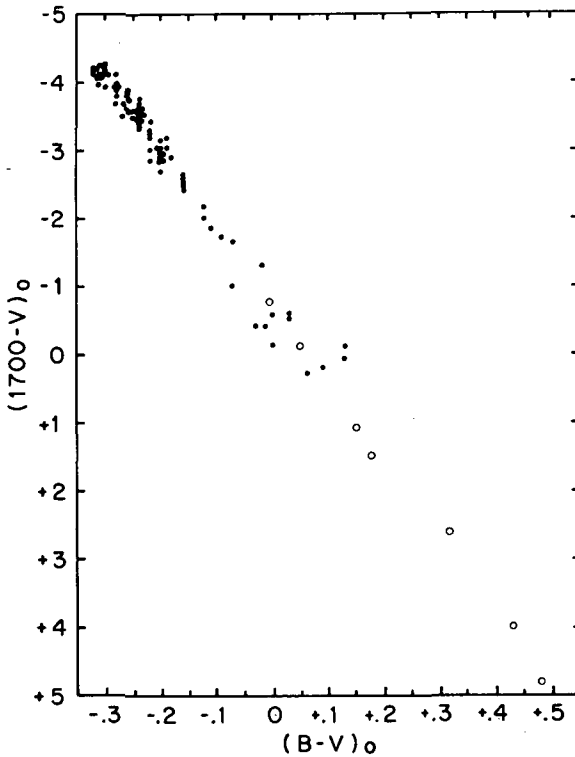


Figure II-7 Stellar $\lambda 1700 - V$ color vs. $B - V$ with 1700 \AA wide-band photometry from OAO.

simultaneous observation of many image elements. Nevertheless, different, generally more restrictive kinds of observations will be necessary for cool stars.

It is possible that the already large factor of 10^4 decrease in flux we have seen in Figure 6 will not become greater for certain observations made at wavelengths shorter than 1700 \AA or for cooler stars. In the Sun the strongest chromospheric lines between 1700 \AA and the Lyman limit produce about the same photon flux as 1 \AA of the continuum near 1700 \AA . If stars of later type than the Sun have chromospheric temperatures more nearly like the solar chromosphere, then the detection of their strongest emission lines might be possible with the same effort required to observe solar-type stars, for which the factor 10^4 applies roughly to all strong lines.

Given the much greater difficulty of obtaining ultraviolet data for cool stars, perhaps some theoretical work might be directed toward the question of which specific ultraviolet measurements would be most helpful in understanding the nature of stellar chromospheres. Guidelines of this sort could prove very useful for the efficient selection and design of future ultraviolet experiments.

Preparation of this paper was supported, in part, by NASA NAS 5-1348 contract.

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DISCUSSION FOLLOWING TALKS BY PRADERIE AND DOHERTY

Kuhi — Now I'd like to call on Rottman to give you a summary of his uv spectral work on Arcturus.

Rottman — I would like to discuss an ultraviolet spectrum of Arcturus obtained from a sounding rocket flight. This experiment was a sequel to one which identified the Ly α emission as reported in *Ap. J.* **165**, 661, 1971. In the present experiment, definite emission lines were observed in the spectral region 1200 Å to 1900 Å. It is expected that such

emission lines will give unambiguous evidence of the existence of and detailed information on chromospheric type layers. This work will be published by Warren Moos and myself.

Kuhi — I'd like for Kondo to present his work on high resolution scans of the Mg II resonance doublet in late type stars.

Kondo — This work was done in association with Tom Giuli and A.E. Rydgren of the NASA Manned Spacecraft Center and Jerry Modisette of Houston Baptist College. We report the initial results of a balloon-borne experiment designed to investigate emission of the Mg II resonance doublet in stars. The Mg II resonance doublet at 2795.5 Å and 2802.7 Å ($3s\ ^2S - 3p\ ^2P^\circ$) is the ultraviolet magnesium counterpart of the Ca II resonance doublet at 3933.7 Å and 3968.5 Å ($4s\ ^2S - 4p\ ^2P^\circ$). For certain spectral type stars the Ca II doublet has been observed in emission, which is believed to indicate chromospheric activity in these stars.

The Earth's atmosphere is opaque to radiation at 2800 Å, and until recently the Mg II doublet emission had been observed only in the solar spectrum, by means of rocket-borne and satellite payloads. Comparison of the Ca II and Mg II emission in the solar spectrum indicates that the latter is by far the more distinct and prominent of the two.

There are several theoretical reasons why the Mg II emission should be more prominent than the Ca II emission, at least for certain spectral types. First, the cosmic abundance of magnesium is about 17 times greater than that of calcium (Allen 1963). Second, the ionization and excitation potentials of magnesium and calcium are such that in A and F stars, the Mg II resonance lines are nearer to their maximum strength than are the Ca II resonance lines. Thus, for these stars one expects deeper and wider absorption lines for Mg II than for Ca II, which makes weak emission in the line bottom easier to detect. Third, for stars of spectral type later than A, the continuum level of 2800 Å is lower than at 3950 Å, facilitating detection of any weak emission. The Ca II doublet emission becomes difficult to observe in stars earlier than mid-F, and one of the objectives of this experiment is to determine whether the difficulty is due to observational limitation or to the disappearance of the mechanism responsible for the chromospheric emission. The other objective of this experiment is to survey the behavior of the Mg II resonance doublet in stars of various spectral types.

Recent low resolution UV spectrophotometry from OAO-2 (Doherty, 1971) and from a rocket (Kondo 1972) show Mg II doublet in unresolved emission for stars later than K2.

The current experiment was conceived to scan the Mg II doublet with spectral resolution of at least 0.5 \AA for emissions anticipated for F-type dwarfs brighter than $m_v \approx 5$. It was felt that the 0.5 \AA resolution would be required to unequivocally detect weak emission and also to study the detailed structure of stronger emission lines. The 0.5 \AA resolution is a compromise between high resolution and available observing time. Current operational balloons can carry a sizeable telescope to altitudes approximating 40 km and can maintain those altitudes for an entire night. At these altitudes the atmospheric extinction for $\lambda 2800$ radiation is approximately 50% (Goldberg 1954), so one can expect balloon payloads to have decided advantages over rocket payloads for observations in this wavelength range.

Our payload consists of a 40 cm Cassegrain telescope with an Ebert-Fastie spectrometer, a three-stage star acquisition and tracking system, command and telemetry electronics, and structural components. Figure II-8 is a drawing of the assembled payload. A sketch of the instrument portion of the payload is shown in Figure II-9.

For acquisition of a target star, the telescope is pointed to within $1^\circ.5$ of the star in azimuth by referencing a two-element magnetometer to the horizontal component of the Earth's magnetic field. The telescope is pointed to within $0^\circ.5$ of the star in elevation by means of a position-sensing potentiometer referenced to local vertical. The platform star tracker acquires and centers the target star, which need not be the brightest star in the 1° by 3° field of view of this star tracker. This star is then tracked by the platform star tracker with an accuracy of ≈ 1 arc minute.

A dichroic filter located behind the primary telescope mirror reflects into the spectrometer the light from the star which is in a narrow band of wavelengths centered at 2800 \AA . The visible light from the star is transmitted through the dichroic filter to an image position sensor. Position signals from this sensor are used to control the movable secondary mirror to maintain a fine-pointing accuracy of ± 1 arc seconds. The spectrometer grating has 2160 lines per mm and gives a second-order spectrum with a dispersion of 3.3 \AA mm^{-1} . The detector for the spectrometer is an ITT F4012 image dissector tube with a $\frac{1}{4} \text{ \AA}$ "slit". The spectrum is scanned repetitively in $\frac{1}{4} \text{ \AA}$ steps with scan lengths of 4 \AA , 24 \AA or 50 \AA . The appropriate scan length is chosen in real time and placed anywhere in the spectral range 2775 to 2825 \AA by command from the ground. For further details regarding the payload and instrument, see Kondo et al. (1972), Gibson et al. (1972) and Wells, Bottema and Ray (1972).

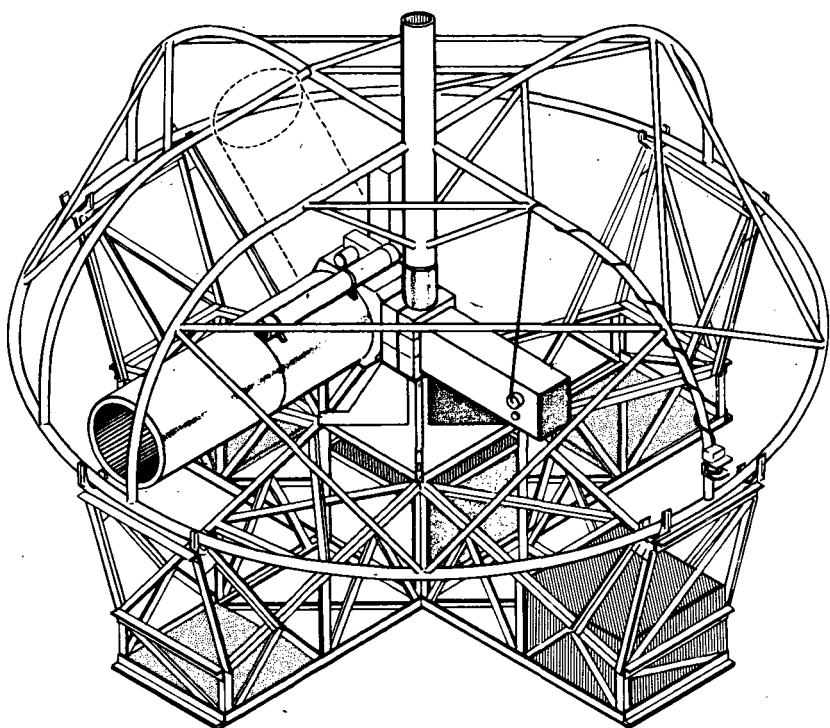


Figure II-8 Balloon-borne ultraviolet stellar telescope-spectrometer payload.

The payload is carried to an altitude of 40 km by a 430,000 m³ polyethylene single cell balloon. Observations are begun after payload sunset and continue until payload sunrise or until the payload drifts out of telemetry range (600 - 650 km from the ground station). The zenith obscuration at float altitude due to the balloon has a radius of 27°. The ground station at the launch site maintains continuous telemetry and command communication with the payload.

For target acquisition, it is necessary to provide the elevation and azimuth angles of the star relative to the payload's local vertical and magnetic north respectively. The latitude and longitude of the payload are monitored by means of the DOD Omega navigation system. The necessary calculations for target acquisition are performed in the ground station using a desk-top digital computer while observing another star. Normally, less than ten minutes are required to perform the calculations, transmit the commands and acquire the target star.

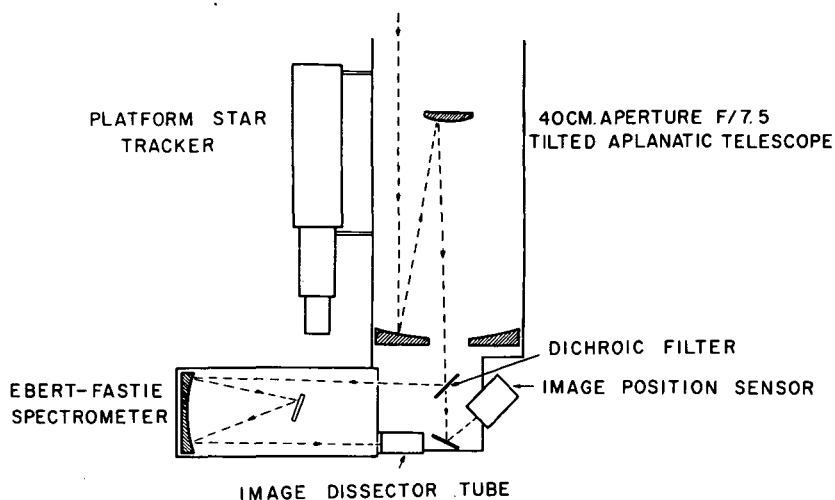


Figure II-9 Schematic telescope and spectrometer layout.

Once a target star is acquired, the scanning of the spectrum is begun and the data are telemetered to the ground station. The accumulated spectrum is displayed on a large oscilloscope so that the investigators can watch the counts build up, and the data are simultaneously recorded on magnetic tape for subsequent analysis. The oscilloscope data display allows the investigators to make real-time decisions regarding scan mode and length of observing time for each star.

Our raw data were in the form of counts per 50 milliseconds per $\frac{1}{4}$ Å channel. The magnetic tapes containing the data were analyzed to separate and accumulate the data for each star and to give the wavelength calibration and background count information. Using in-flight scans of an on-board wavelength reference lamp, we have calibrated our wavelength scales to an accuracy of $\pm \frac{1}{4}$ Å. Corrections for the Earth's orbital motion have been applied to reduce the observed wavelength scales to the Sun. No corrections for sky background and dark count have been made, since with the possible exception of the continuum of α Ori, they were negligible compared with the stellar flux.

From laboratory measurements and the analysis of in-flight wavelength reference line profiles, we have determined our resolution to be between 0.25 and 0.5 Å. Except as noted for γ Lyr, our data are presented in the form of observed counts per $\frac{1}{4}$ Å channel. The statistical uncertainty of each data point is the square root of the plotted value.

For the ϵ Lyr data, an alternate approach was made to the error analysis, by generating Monte Carlo simulations of the spectrum taking into account both counting statistics and smearing in wavelength. The results with regard to identification of features were not significantly different from the conclusions indicated by the error bars in Figure II-10.

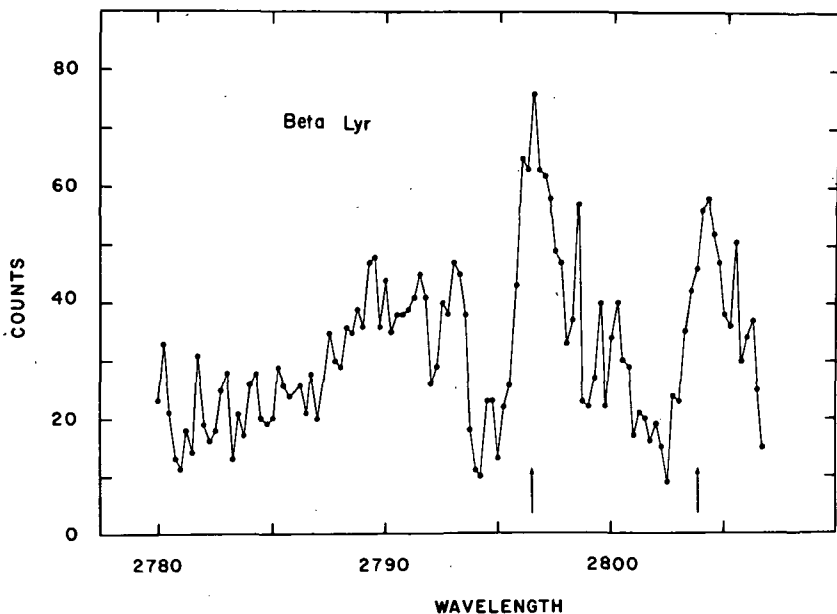


Figure II-10 Observation of the Mg II lines in β Lyr on 1971 June 6/7. The arrows indicate the Mg II line centers at the radial velocity of the B-star component at the time of observation.

We have thus far observed β Lyr, γ Lyr, β Cas, α CMi and α Ori. The first two were observed on the night of 1971 June 6/7, and the latter three were observed on the night of 1971 October 7/8.

β Lyr (*Bpe*, $m_v = 3.7v$) — The well-known eclipsing binary β Lyr was observed near 5^h UT on 1971 June 7. The presence of numerous emission features in the visible spectrum of β Lyr suggested that it would be a likely candidate for interesting spectral features involving the Mg II resonance doublet. This was borne out by our observations. One representative scan of β Lyr is shown in Figure 3. This shows broad overlapping emission features with deep absorption on the short-wavelength sides of the emission peaks. The line profiles are similar to the profiles of the emission lines in the visible spectrum of this star.

Using the ephemeris of Wood and Forbes (1963), we compute the phase of our observation to be OP.89. The radial velocity curve of Abt (1962)

gives a radial velocity of $+120 \text{ km sec}^{-1}$ for the B-star component at the phase of our observation. The line centers of the Mg II doublet in the B-star are near the tops of the emission peaks. The two absorption features are about 2 \AA or 200 km sec^{-1} in width. Abt determined the γ -velocity of the system to be $-19.5 \text{ km sec}^{-1}$. The line centers at the γ -velocity of the system are located in the deep absorption features.

We note that the emission spikes longward of the two emission peaks are statistically significant and are displaced equal amounts from the B star line centers. We obtained three other 50 \AA scans of $\beta \text{ Lyr}$ along with several partial scans. Intercomparison of this data suggests that there were significant changes in the emission portions of the features on a time scale of tens of minutes.

$\gamma \text{ Lyr}$ (B9III, $m_v = 3.3$) — We observed $\gamma \text{ Lyr}$ briefly during the first flight to confirm the accuracy of the pointing system of the payload. In the scan mode used to observe $\gamma \text{ Lyr}$, the time required to obtain 8 counts per $\frac{1}{4} \text{ \AA}$ channel was measured. Because of the low accuracy of this data, we have averaged this data over $\frac{1}{2} \text{ \AA}$ intervals. These data points have been converted for display purposes to the number of counts which would have been observed in 3.2 seconds per $\frac{1}{4} \text{ \AA}$ channel. The resulting scan of $\gamma \text{ Lyr}$ is shown in Figure II-11. The statistical uncertainty of each plotted point is 25% of the plotted value.

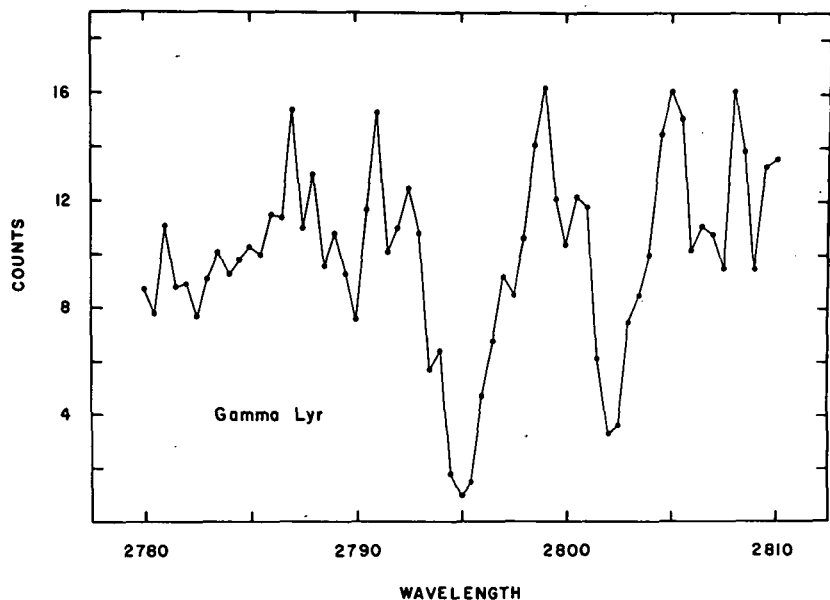


Figure II-11 Observation of the Mg II lines in $\gamma \text{ Lyr}$ on 1971 June 6/7.

The Mg II resonance doublet in γ Lyr appears as two deep, separated absorption lines. Correction for the stellar radial velocity of -22 km sec^{-1} (Hoffleit 1964) places the line centers at the observed absorption minima. The 2795 Å line is deeper and wider than the line at 2802 Å, as expected from the statistical weights. Although the exact continuum level is somewhat uncertain, the residual intensity in the bottom of the 2795 Å line appears to be about 0.1. There is no evidence for any emission associated with the Mg II lines in this star.

One objective of this project is to look for Mg II emission in early and mid F stars along the main sequence. Wilson (1966) studied rotational velocities and Ca II H and K emission along the main sequence between $b-y = 0.240$ and $b-y = 0.440$. He determined the regions of fast and slow rotation in the c_1 ($b-y$) diagram as shown in Figure II-12. Wilson found that the "fast rotation" region contained some slow rotators, while the "slow rotation" region contained no fast rotators. The boundary between the regions intersects the zero-age main sequence (ZAMS) near $b-y = 0.285$. Using 10 Å mm^{-1} Coude spectra, Wilson detected Ca II emission only as early as $b-y = 0.304$, although he suspected that higher-dispersion spectra might show Ca II emission as early as $b-y = 0.275$. We observed the Mg II resonance lines in the main-sequence F-stars β Cas and α CMi during the second flight. Both stars are plotted in Figure II-12 on the basis of the uv by photometry in the Stromgren-Perry Catalog (Stromgren and Perry 1965).

β Cas (F21V, $m_v = 2.2$) — Using the absolute magnitude calibration of Stromgren (1963), we find that β Cas is about $1^m.4$ above ZAMS. The Mg II resonance lines in β Cas (Figure II-13) appear as broad overlapping absorption lines with distinct minima. The 2795 Å line is deeper than the 2802 Å line. There is no prominent Mg II emission in this star. In order to investigate the existence of weak emission in the 2795 Å component we have smoothed the data over successively more channels in Figure II-14. The curves demonstrate that, even when the data are smoothed over 1 Å, the possible emission is still apparent. The stellar radial velocity is $+12 \text{ km sec}^{-1}$ (Hoffleit 1964). (Although β Cas is listed by Hoffleit (1964) as a spectroscopic binary, Abt (1970) finds no convincing evidence for this.) It is interesting to note that the low data points at 2795.5 Å are at the expected line center and might be a " K_3 " component. The flat residual intensity which occurs in the bottom of the 2802 Å line may constitute weak emission at this wavelength.

α CMi (Procyon F51V, $m_v = 0.3$) — According to the Stromgren-Perry catalog photometry and Stromgren's (1963) calibration, Procyon is about $0^m.4$ above ZAMS. Procyon's $b-y$ value of 0.272 places it just outside Wilson's slow rotation region, but he classified it as a slow rotator. Kraft

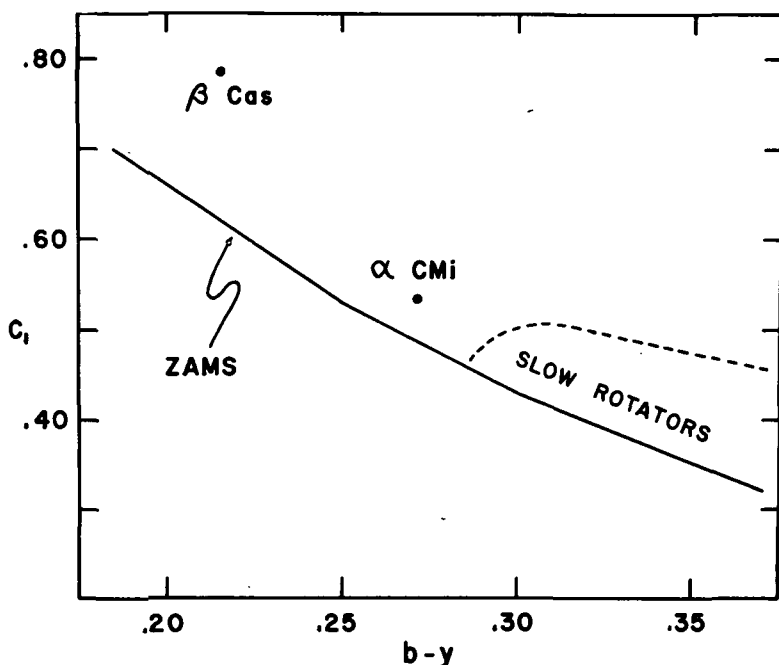


Figure II-12 c_1 -($b-y$) diagram for F stars. The solid line is Stromgren's (1963) Zero Age Main Sequence. The broken line is the boundary between Wilson's (1966) fast and slow rotation regions.

and Edmonds (1959) found "feeble but definitely present" Ca II emission in Procyon, using 3.2 and 4.8 \AA mm^{-1} spectra. They noted that the short-wavelength side of the emission appeared stronger than the long-wavelength side. Our microdensitometer tracing of the Coude plate of Procyon provided by O.C. Wilson (Figure II-15) also shows a similar weak Ca II K emission feature. Recently, Linsky (1972) observed similar K emission. Our Mg II observations of Procyon appear in Figure II-16. Procyon has a faint companion ($B\omega \Lambda 10$) with an orbital period of about 40 years. The γ velocity of the system is 4 km/sec and the semi-amplitude of the spectroscopic orbit is only 1.3 km/sec (Jones 1928). Thus the true Mg II line centers should be at $2795\frac{1}{2}$ and $2802 \frac{3}{4} \text{ \AA}$. The most noticeable difference between the Procyon and β Cas Mg II lines is the distinct emission which appears in the Procyon lines. The emission feature at 2795 \AA is asymmetrical, with the stronger emission on the short-wavelength side, analogous to the Ca II observation in Procyon.

α Ori (Betelgeuse M2Iab, $m_v = 0.8v$) — Our Mg II observations of the supergiant α Ori are presented in Figure II-17. This shows both of the Mg

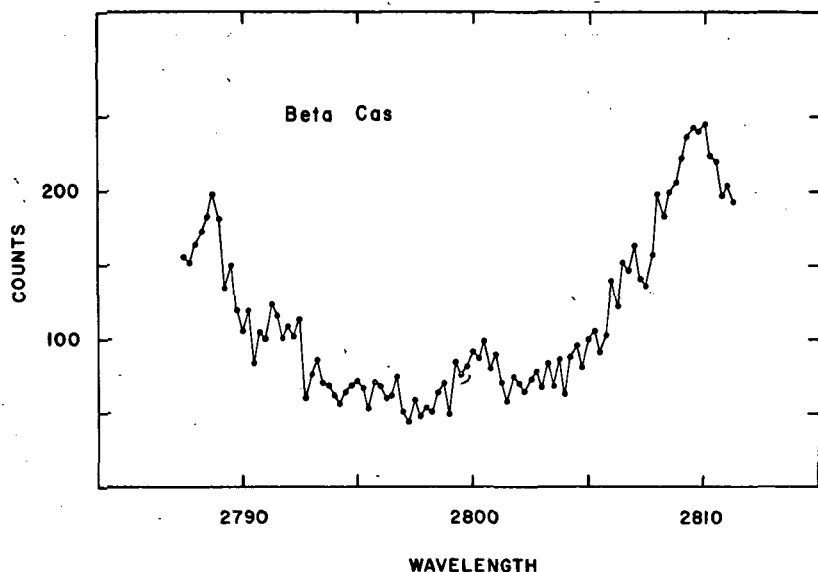


Figure II-13 Observation of the Mg II lines in β Cas on 1971 Oct. 7/8.

II resonance lines dramatically in emission, with each line showing prominent self-reversal. Figure II-18 shows a microdensitometer tracing of the vicinity of the Ca II K line in α Ori from a Coude plate loaned by O.C. Wilson. The Mg II emission is far more pronounced than the Ca II emission in this star.

The Mg II line centers corrected for the stellar radial velocity of + 21 km sec⁻¹ (Hoffleit 1964) should be located at 2795 $\frac{3}{4}$ and 2803.0 Å. The observed self-reversal minima are located at 2796.0 and 2803.5 Å. Considering our wavelength calibration uncertainty of $\pm \frac{1}{4}$ Å and our resolution of between 0.25 and 0.5 Å, it is not clear that the observed separation of the "K₃" and "H₃" minima is significant. We note that the "K₃" minimum in α Ori is deeper than the "H₃" minimum.

One of the striking features of the Mg II emission in α Ori is that the 2802 Å line is almost perfectly symmetric, while the 2795 Å line is definitely asymmetric. The height of the shorter-wavelength "K₂" peak is significantly lower than the height of the longer-wavelength "K₂" peak, although the "K₃" minimum is centered on the emission feature. Our planimetry of the two lines shows that the area under the 2802 Å line is about 12% greater than the area under the 2795 Å line. This difference in line shape between the 2795 and 2802 Å components is most puzzling. We are not sure how much of the count level outside the emission is true stellar continuum and how much is background count.

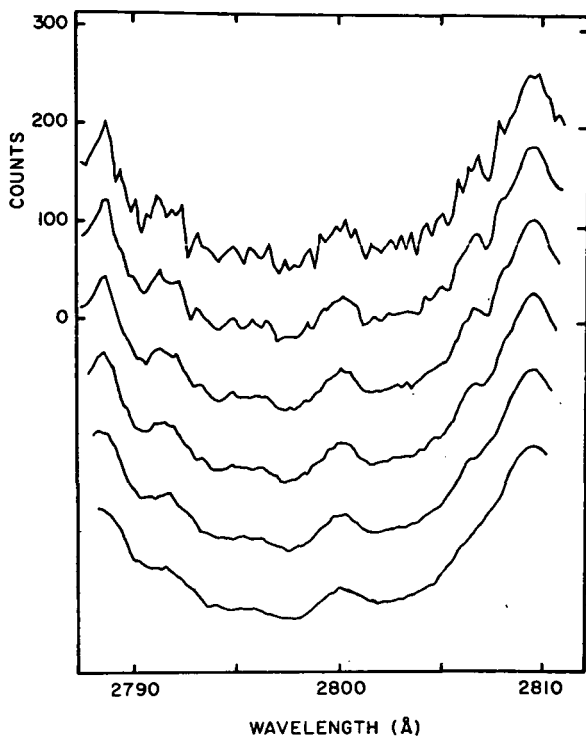


Figure II-14 The observation of the Mg II lines in β Cas smoothed successively over 1, 2, 3, 4, 6 and 8 channels.

We have measured the widths of both Mg II emission features in α Ori, the width of the 2795 Å emission in α CMi and the width of the possible 2795 Å emission in β Cas. We have also estimated the width of the 2795 Å emission in the solar spectrum from the published data of Purcell et al. (1961) and Lemaire (1970). Our estimates of the Mg II emission widths and their uncertainty are given in Table II-1.

TABLE II-1

| Mg II Emission Widths | | |
|-----------------------|-----------|-----------------|
| Star | Width (Å) | Uncertainty (Å) |
| α Ori | 3 3/4 | $\pm 1/4$ |
| β Cas | 2 1/2 | $-1/4, +1/2$ |
| α CMi | 1 1/2 | $-1/4, +1/2$ |
| Sun | 0.7 | ± 0.1 |

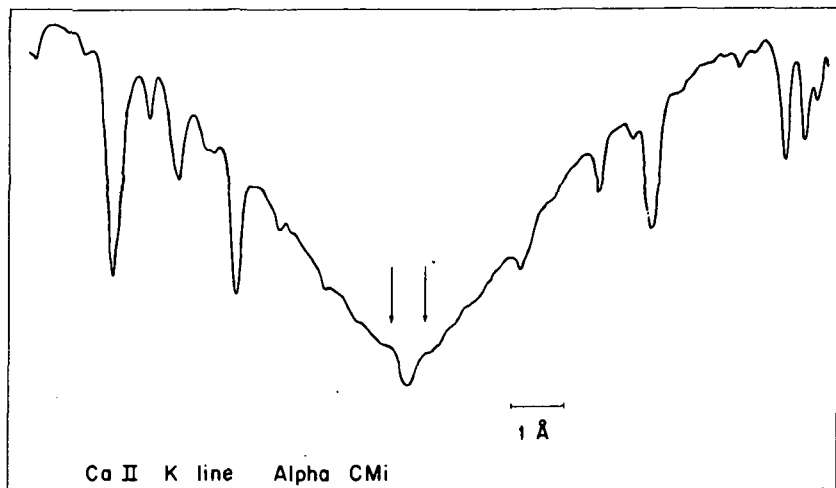


Figure II-15 Microdensitometer tracing of the Ca II K line in α CMi
The arrows indicate the K₂ peaks.

Figure II-19 is a plot of absolute visual magnitude versus $\log W$, where W is the full width of an emission line at its base in km sec^{-1} . This figure shows the Wilson-Bappu (1957) relationship between M_v and $\log W$ for Ca II K emission. On this figure we have superimposed our Mg II emission widths with error bars. Excluding β Cas, for which we are not certain that there is emission, we find that the Mg II emission widths are wider than the corresponding Ca II widths by $\Delta \log W \approx 0.4$. The present data are too limited to indicate definitely whether or not there is a unique relationship in this diagram from Mg II emission which is essentially independent of spectral type and emission strength, as is the case for Ca II.

The difference in width between the Ca II K and Mg II 2795 Å emission lines probably depends in part on the greater abundance of magnesium over calcium. However, it may also depend on the heights at which these "collision-controlled" lines are formed. The higher excitation and ionization potentials of magnesium provides an argument for Mg II lines being predominately formed at higher temperature and hence higher altitudes in the stellar chromosphere. An additional argument for Mg II line formation at higher altitudes is the increased optical thickness of the line due to the greater magnesium abundance. If the lines are formed at higher altitudes, then either increased turbulence, Doppler spreading due to a progressive increase in the radial flow velocity (if there is a stellar wind), or diffusion

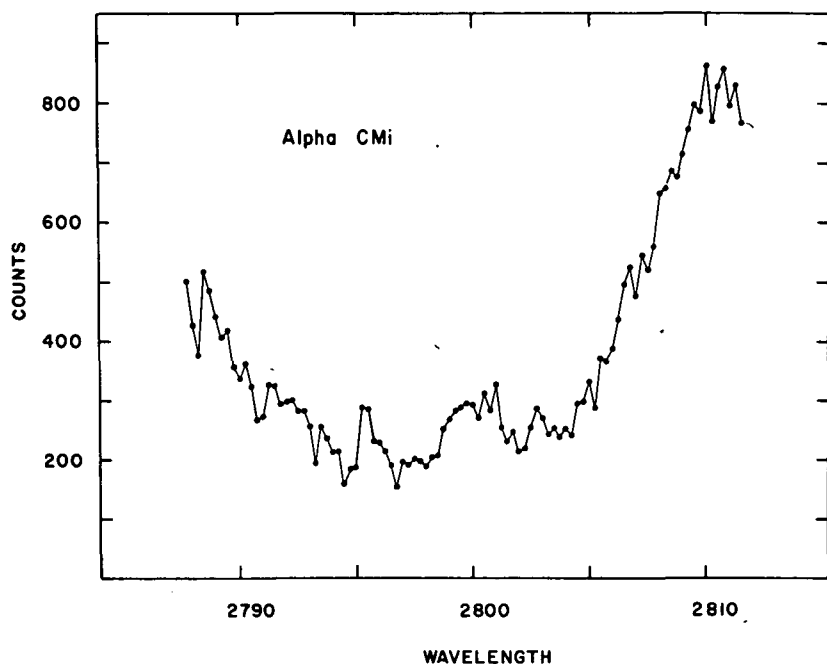


Figure II-16 Observation of the Mg II lines in α CMi on 1971 Oct. 7&8.

of the photons in wavelength for an optically thick line center could work to increase the line width.

We wish to thank the many people who supported us in the design, fabrication, testing, and flight support of the instrument. Finally, we would like to thank Dr. O.C. Wilson for making available his ground based Coude plates for comparison with our balloon observations.

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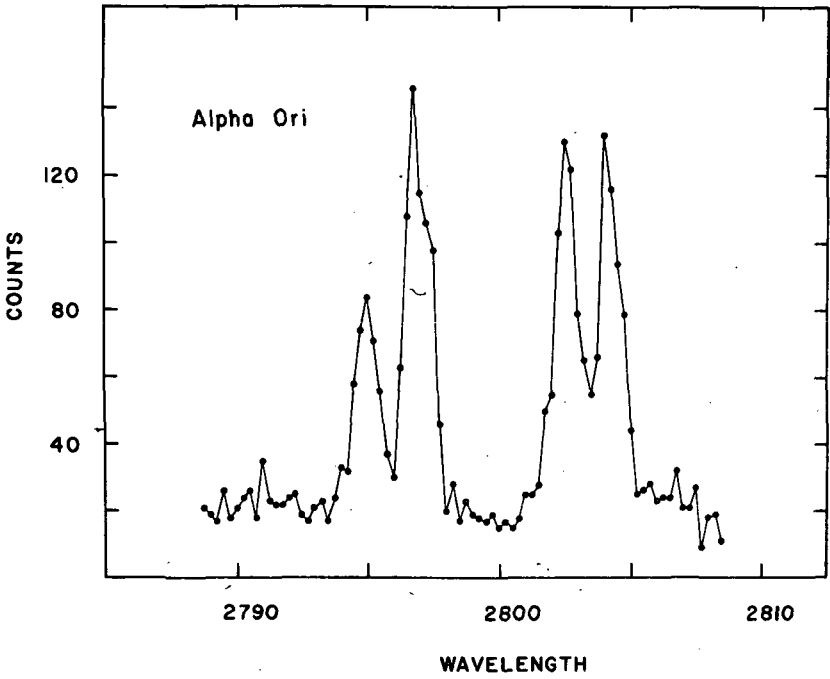


Figure II-17 Observation of the Mg II lines in α Ori on 1971 Oct. 7/8.

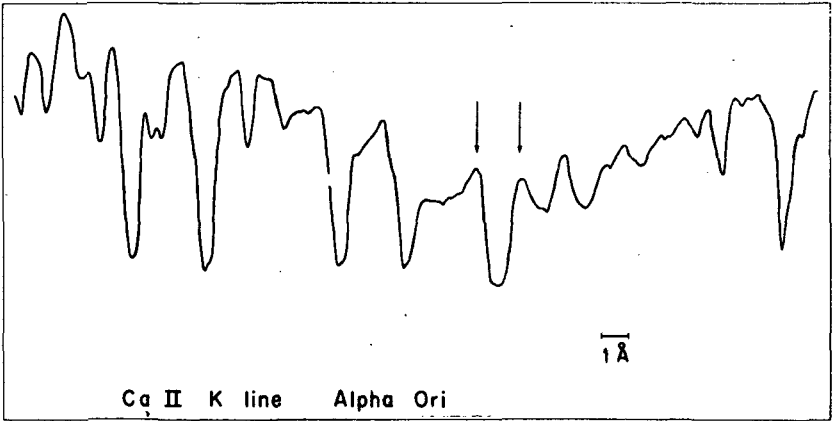


Figure II-18 Microdensitometer tracing of the Ca II K line in α Ori.
The arrows indicate the K_2 peaks.

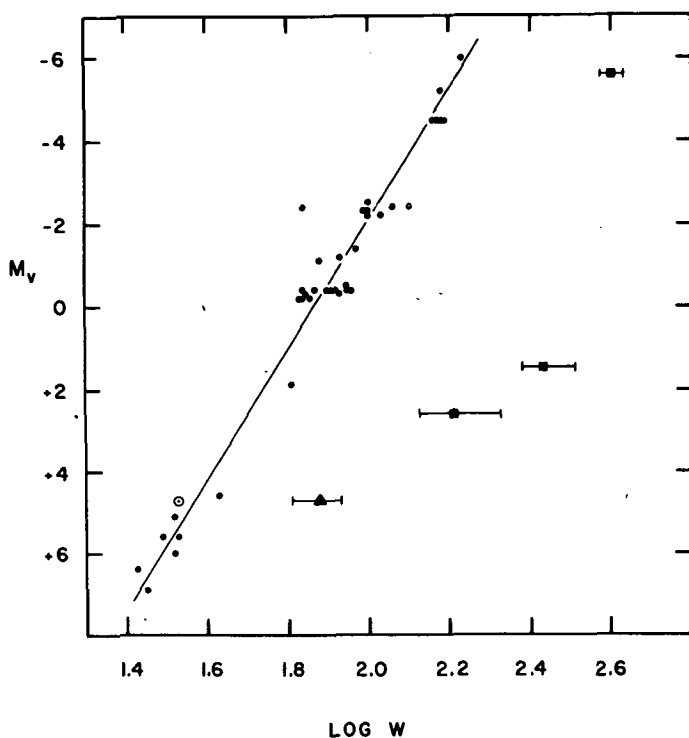


Figure II-19 Plot of M_v versus $\log (W)$, where W is the width of an emission line in km sec^{-1} . The filled circles are Ca II widths from Wilson and Bappu (1957). The dotted circle is the Ca II width in the Sun (Wilson and Bappu 1957). The squares are Mg II widths in α Ori, β Cas and α CMi respectively, in order of decreasing luminosity. The triangle is the solar Mg II width.

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CONTINUATION OF DISCUSSIONS FOLLOWING TALKS BY PRADERIE AND DOHERTY

Kuhi — Now let's have a general discussion of Francoise Praderie's paper. Let's first discuss the question of what we do mean by a chromosphere from an observational point of view. One thing that bothers me a great deal is the distinction between a stellar chromosphere as we've come to think of it in the Sun and the changes that seem to take place as one goes from cool stars like the Sun to hotter and hotter stars in which the distinction between the defining characteristics becomes ever more vague, in separating out a chromosphere, an extended atmosphere, an extended envelope, and so on.

Aller — I think it is very important to make, as you say, a distinction between a chromosphere on the one hand, and what have loosely been called extended envelopes and shells on the other. There are a number of objects in which the gradation from one to the other is certainly not clear cut. A good example is RR Telescopii. In that star you see a spectrum of ionized titanium and iron that looks qualitatively somewhat like the flash spectrum of the Sun. Superimposed on it, however, are increasingly higher levels of excitation; both forbidden and permitted iron lines, ranging on up from [Fe II] to [Fe VII]. In fact, [Fe VII] supplies the strongest features in the emission spectrum of this object. In looking at the spectrum carefully there seems to be no place where you can say everything of one or two levels of ionization should be assigned to an ordinary chromosphere and everything else is to be attributed to something else. There seems to be a steady gradation in excitation. It's almost as though we were looking at the solar spectrum, in the near UV region.

Steinitz — I would like more clarification of the definition of a chromosphere. One of the necessary conditions was defined to be mass flux, and it wasn't clear whether the idea was mass loss, or accretion, or just mass motion. Also could you clarify what exactly is meant by non-radiative energy transfer? Should this include or exclude specifically convection?

Praderie — I did not want to include mass loss as such as a necessary condition for a chromosphere, because I have no clear evidence that the

mass loss is unequivocally bound to the existence of a specific region in the atmosphere. One can find mass loss as shown from the shape of profiles in lines of photospheric origin in some stars, whereas, in other stars, the mass loss is expected to occur only in the corona. So the mass loss itself I did not include in my discussion. Mass flux was meant as any net transport of matter in a certain region, of which maybe the mean value over time or over some distance can be zero. Now, concerning the non-radiative energy transport, it is not restricted to the chromospheric layers. In the photosphere you can have it too (turbulent, progressive waves, convection, etc.), but there is no dissipation to heat the thermal pool at that very place. So then I call chromospheric the region where the dissipation starts to act.

Steinitz — It is not clear how the observables which you discussed are directly connected, even though they were classified as direct and indirect observables with those criteria just now mentioned.

Praderie — I am aware that I have not clearly made a bridge between what one would wish to do, according to the scheme which was given in the introduction, and all the detailed observations which are available now. This is a diagnostic task which is far from being completed.

Athay — I think the question of the definition of chromosphere is very critical. We ought to use a definition that will allow us to talk about chromospheres with the least amount of confusion. I think that the way we defined it yesterday and today would lead to a maximum amount of confusion. The proposed definition requires a very careful interpretation of data and is not one which you can very easily go to from observations. We should define chromosphere for use in the literature as requiring the minimum amount of interpretation of data. I think it ought to be defined in terms of temperature reversal which you can at least hope to get to in a simple way. I don't see how an observer could ever get to an observable of mechanical energy flux. So, if we use that as the defining characteristic we'd have to restrict the observers from ever using the term chromosphere, leaving it only for the use of theoreticians.

Kuhi — By mechanical energy flux do you exclude mass loss then?

Praderie — I exclude it, maybe for convenience. In reply to Athay, I admit that we have apparently confused things by giving a definition which is bound to theoretical considerations; but it is my feeling that only from properly analyzed observations can you presume the presence of a chromosphere. I tried to show that if you have a positive outward temperature gradient it doesn't tell you enough. Even if you have none, you may miss the start of the chromosphere. I think one has to look for general definitions, not only for operational ones.

Thomas — Here I also disagree with Athay. Let me give you two examples. It seems to me that we should be defining things which the observers can use unambiguously when they look at data. My two examples are the atmosphere of the Sun and the atmosphere of a $15,000^\circ$ star. The basic question for the interpretation of stellar atmospheres is, is it sufficient just to drop the assumption of LTE? Or must I also drop the assumption that there are only radiative energy sources? To me, a chromosphere is that atmospheric region for which I must drop the assumption of radiative equilibrium. This is very clear conceptually. From a purely observational standpoint what then is the situation? In the Sun, at $\tau = 1$, I have a temperature of about 6000° . I have a temperature minimum of about 4200° , judging from the observations. At a height of about 500 km in the chromosphere, the temperature is again about 6000° . Now the maximum temperature one would get from radiative processes alone is about 5300° , based on the work of Cayrel, Frisch and others. Hence, for the Sun, we can infer the input of non-radiative energy. Now for the $15,000^\circ$ star, pure continuum models give a maximum boundary temperature of about 9500° , based upon the work of Auer and Mihalis and the simple calculations of Gebbie and Thomas. The introduction of the effect of lines on populations may raise this value as high as $13,800^\circ$. The clear cut observational question to be answered, then, is do the temperatures prevailing outward from the temperature minimum of the $15,000^\circ$ star exceed the value predicted from radiative equilibrium models? If so, we can infer the dissipation of non-radiative energy and hence the existence of a chromosphere.

Conti — I would like to take a heretical view of the chromosphere by defining it in a simple way. Suppose we say that any time you see emission lines you have evidence for the existence of a chromosphere.

Kuhi — How would that allow one to distinguish between chromospheres and large scale extended atmospheres?

Conti — Maybe there is no essential difference, except in the scale. If a theoretician tells me that a chromosphere is present, I know that I'll see emission lines. The only question that remains is, if you see emission lines in Wolf-Rayet stars, Of stars, or early A or B stars, does it necessarily imply the existence of a temperature rise, mechanical heating or mass loss? I don't wish to go into a detailed theoretical discussion on this, but, as far as I know, where emission lines are seen, at least one of these three phenomena is always present. So we could have, as a working definition, that a chromosphere is a region in a stellar atmosphere which gives rise to emission lines.

Kuhi — Are there contrary views? I believe the problems for both the observer and the theoretician are much worse than Dick indicated.

Underhill — I agree with Conti. However, I believe the problems for both the observer and the theoretician are much worse than Dick indicated.

Kondo — With regard to Conti's definition, I wonder if you would include close binaries in this category. They do have different problems than other stars such as those involving mass transfer and mass loss. Our balloon observations and OAO-2 observations show that β Lyrae has magnesium doublet emission, for example.

Conti — One could make exceptions but one could also use these to illustrate the point. There are close binaries which have greatly enhanced H and K emission. λ Andromedae is a fine example. Its emission lines are certainly chromospheric. And so we see that the chromospheric phenomenon has been accentuated by heating in a close binary.

Underhill — My definition of a chromosphere is that region of a stellar atmosphere that deviates from a simple model. Figure II-20 shows the predicted flux envelope for an ordinary $13,000^\circ$, $\log g = 4.0$ model

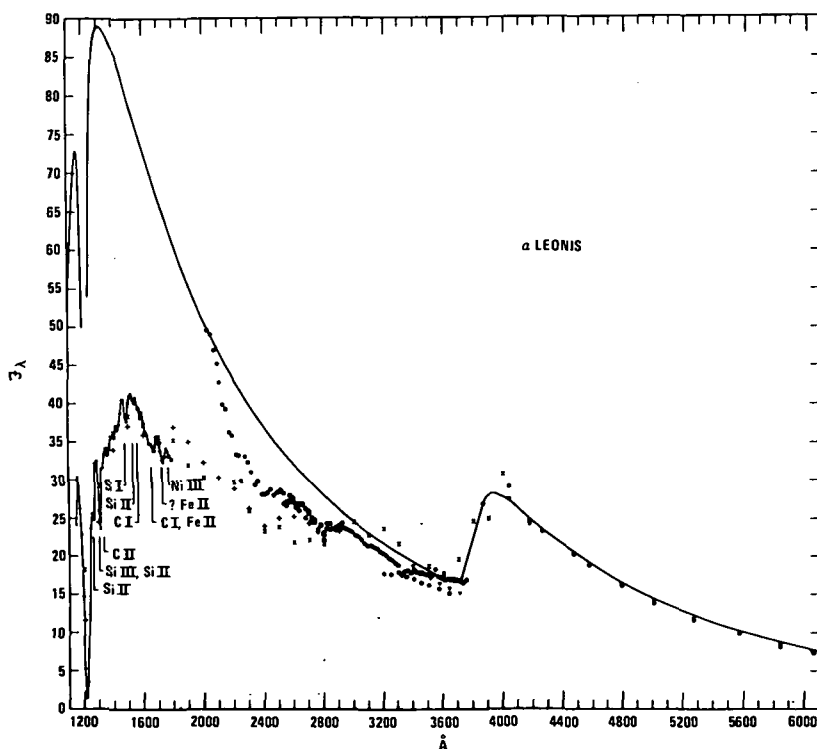


Figure II-20

atmosphere, calculated in hydrostatic equilibrium, in LTE, with the plane parallel assumption, etc. $13,000^\circ$ is a fair choice of effective temperature for a B7 or B6 star like α Leo. The ground-based observed absolute fluxes are in units of 10^{-10} ergs/cm²/sec/Å. As shown the model calculations fit the ground-based data like a glove. Also shown are the UV observations from OAO and from rockets. The OAO scanner 1 observations (3700-1800 Å) are calibrated with the relative sensitivity function given to me by Savage. The rocket observations in the same wavelength range are tied into quite a decent absolute calibration and lie considerably below the OAO observations. I have concluded that the Savage sensitivity function must be in error and I have derived a new sensitivity function by forcing the scanner 1 data to fit the rocket observations. The short wavelength OAO scanner 2 observations have also been calibrated against absolute rocket fluxes. What I want to point out is that up to now we've been talking about the visual part of the spectrum which can be fit well with models, as long as you don't look at the results too closely. But as soon as you get into the ultraviolet below about 2800 Å, the observed flux drops away from the model very rapidly. These results for a B7 star are similar to those I've also found for a B0 and a B3 star.

Something even worse is illustrated in Figure II-21 which shows the observed flux for a rapidly rotating AOV star, γ UMa. The continuous line gives the flux envelope for a hydrogen line blanketed model, effective temperature 9750° , which fits the observations in the visible region. The observed flux shortward of 1800 Å lies very much below the model, indicating line blanketing of a factor of about two. In Figure II-22 is shown the observed flux distribution for Vega, which is also matched to theoretical fluxes in the visible. Now, you see a difference between those two AOV stars, one rapidly rotating and one not. For Vega, we have an excess of flux below 1600 Å, with respect to the reference distribution (that of the model atmosphere), while for γ UMa we have a deficiency of flux with respect to the reference model.

Figure II-23 shows rocket and ground-based observations of α Cma, fitted to the same reference model. $T_{\text{eff}} = 9750^\circ$. Again there is a lot more flux below 1600 Å than you have in the rapidly rotating AOV star, γ UMa, but not as much flux as there is in Vega.

What I really want to say is summarized in Figure II-24. Here are the three AO stars, or A1 in the case of α Cma, plotted with respect to the same model. You get considerable UV line blanketing in γ UMa; α Lyr has a large brightness, or flux excess. It is 50% brighter than γ UMa at 1800 Å or so; and α Cma lies in between. One would never have known that these three stars differ so much, from studying the ordinary ground-based spectral region, to which we have been fitting models. In Vega's far UV

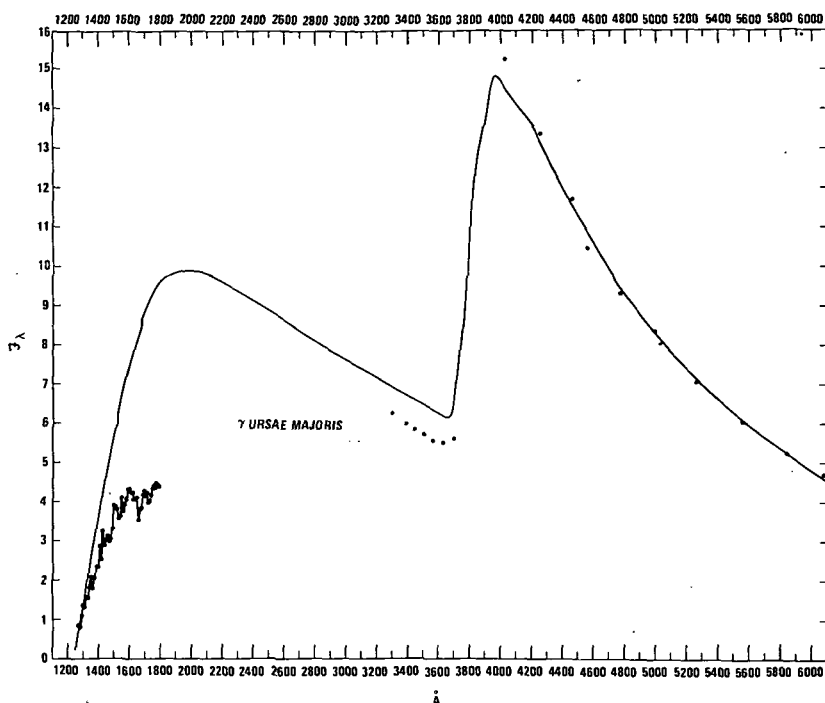


Figure II-21

flux excess are we seeing a hot chromosphere or a companion? I really don't know. γ Lyr is a very funny star; it has been previously postulated to be double. The point is that around $T_{\text{eff}} = 10,000^\circ$, the predicted ultraviolet spectrum is terribly sensitive to the details of the model shortward of 3000 Å. Nothing that we've been able to observe from the ground is nearly as sensitive. So the ground-based observer is up against a real problem in trying to determine if a chromosphere is present or not. Simple classical models predict continuously dropping temperatures and pressures as you go outward in the atmosphere. I defined, half jokingly, a chromosphere as being that region which reflects a departure from such simple models. Unfortunately, most ground-based observables are not very sensitive to these departures.

Hack — I would like to make a comment about the Conti definition of a chromosphere, having in mind the extended atmospheres of A-type supergiants. If we look at spectra of Ia supergiants we see H α in emission, and according to the Conti definition we should say that these stars have a chromosphere. If we look at the spectra of Ib A-type supergiants we generally don't see H α emission. But in both types we observe the same

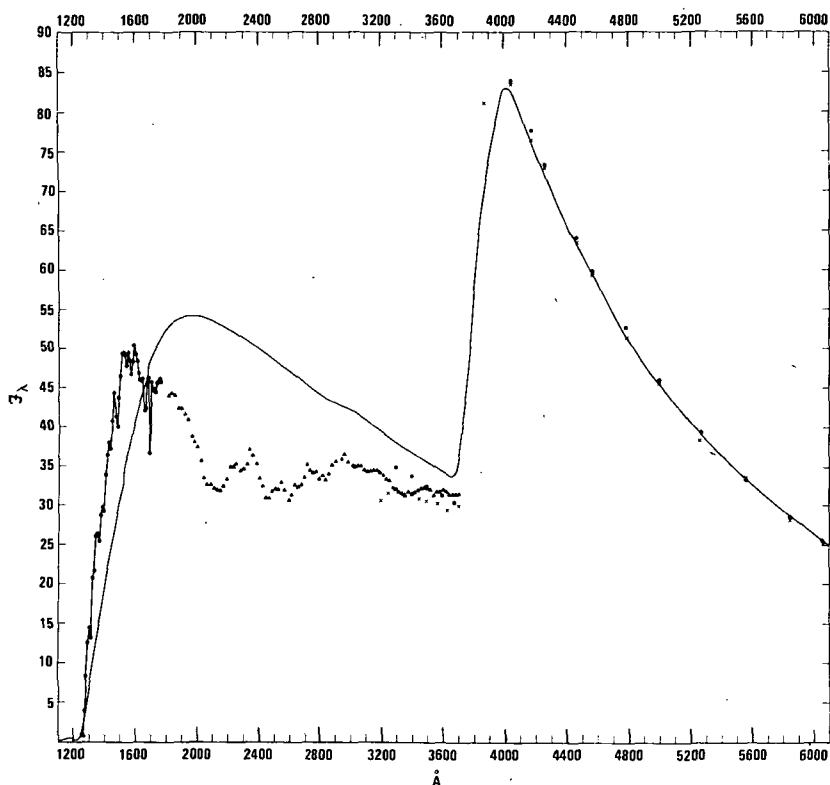


Figure II-22

kind of radial velocity fields, and Balmer velocity progression, which indicate an expanding atmosphere. Hence, in my opinion, we must use the same definition (chromosphere, or extended atmosphere?) for both Ia and Ib atmospheres. The line contours are rather different in spectra of normal B-type stars and in spectra of β Canis Majoris stars, which sometimes show one, two or three components, variable with time and having different radial velocities. So I don't agree that they are equal to those of the normal main sequence stars. As a matter of fact there are some evidences that they are surface rather than atmospheric effects. Huang has shown that the sum of the equivalent widths of the components (measured at phases when the line is divided in two components) is equal to the equivalent width of the line (measured at phase when the line is single). He interpreted this fact as a proof that the components are not formed at different heights in the atmosphere, but rather in different parts of the stellar surface.

Kuhi — I think that is the problem with a definition that says anytime we see lines in emission we have a chromosphere.

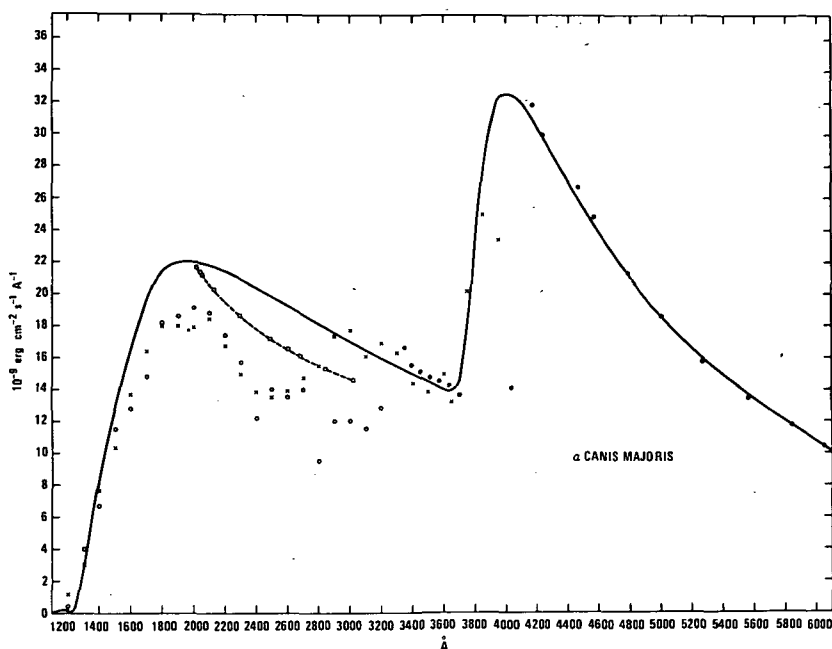


Figure II-23

Cayrel — I am going to propose a very simple definition of a chromosphere, because I think it is too dangerous to have a definition based on assumptions you are making in your work. Very clearly, when people first defined the chromosphere of the Sun, they had the idea that when you look tangentially (at the solar limb) you get an optically thin situation in the continuum. So I would propose that the base of the chromosphere is where you have tangential optical thickness equal to one. There is then the problem of what kind of optical thickness we are using in the continuum. I would propose to define a wavelength λ_0 by $\lambda_0 T_{\text{eff}} = 0.288$, and select a wavelength in the continuum which follows the spectral type or effective temperature. The other problem is what is the upper boundary of the chromosphere. In the word chromosphere you have “chromos” which means color, the idea being that when you look tangentially above this layer you are looking into lines. If there is a dominant line you get the color of this line. I would propose to take as an upper boundary $\tau_{\text{tangential}} = 1$ in the strongest line of the spectrum which may be quite different in a cool star and in a hot star. In the sun I think that would be H α . I don't know what the strongest line would be in hot stars. I think this would eliminate the problem of extended envelopes, because even in lines you are optically thin in envelopes.

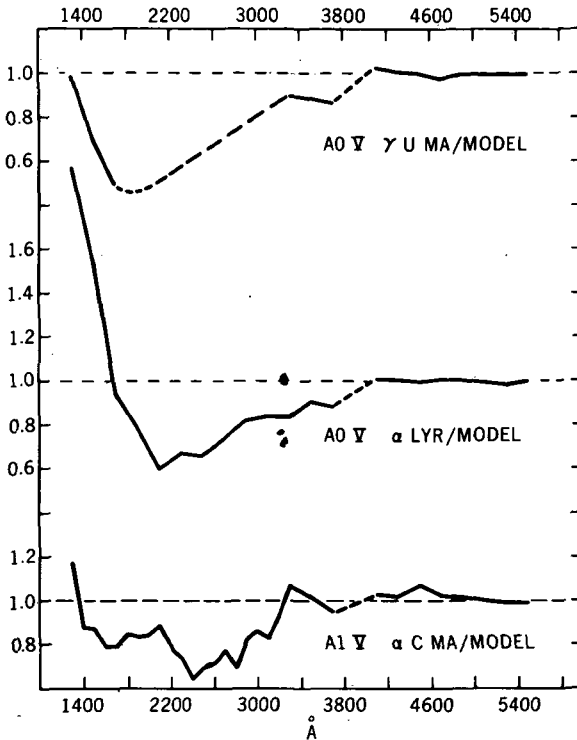


Figure II-24

Kuhi — I'm not so sure that's true. There are stars with large envelopes that have optical depth much greater than one in emission lines.

Cayrel — In that case perhaps the chromosphere merges into the envelope.

Auer — I will be heretical about the definition of a chromosphere. Some objects are interesting because they have a chromosphere. If you ask the average graduate student what the solar chromosphere is he will say it is that region where there is an outward temperature rise. Would someone please tell me what is wrong with that definition. *There are lots of reasons for having emission lines. One of them is a temperature rise*, and that is one phenomenon that I would call a chromosphere. It is the simplest definition. There are problems with definitions that require mechanical heating. After all there are granules in the solar photosphere which are evidence of the presence of a mass flux. Are you therefore going to make the photosphere a part of the solar chromosphere by the mechanical motion definition?

Kuhi — We are dealing with observations here today and I think the question is simply how do we define chromospheres in stars from an observational point of view.

Auer — I think the answer is clearly from phenomena related to a temperature rise. There are different ways to get emission lines, one of which is by means of a temperature rise. Certain lines will show emission because of this temperature rise.

Kuhi — So how do we go backward from observing emission lines to inferring the presence of a temperature rise? That is a little hard to do without good models.

Auer — It is hard to do, but that is not the problem of a clean definition of a chromosphere.

Steinitz — From an observational point of view, couldn't one say that a sufficient condition, not a necessary one, would be that you find emission lines with excitation temperatures higher than the color temperature of the star?

Kuhi — But one can think of stars that don't fit that.

Praderie — We have called a chromosphere a region where we find a temperature higher than that which you would expect in a radiative equilibrium. If we take all emission lines as characterizing a chromosphere, we can get into trouble because some of them, those formed by very specific excitation processes like fluorescence, will say something only about the radiation field and not about the gas kinetic temperature. Secondly, I also suggest that with Auer's definition of the chromosphere as being a region with a temperature rise outward, you have hidden the confusion within the definition, because you do not know what is the cause of the temperature rise at that place. I admit that I have not proved, in any of the indicators I have given, that they say something *directly* about the heating, except in the sense that Steinitz has just stated, i.e., whether the temperature derived is higher than some color temperature in the spectrum.

Frisch — I would like to know why we need a definition of a chromosphere. We need a word that everyone agrees about. Perhaps when we have many observations and people can do statistics, then we will need a definition. But now it is premature.

Kuhi — I don't really want a definition of a chromosphere. I would like to know the answer to the reverse question. If we observe emission lines in a star, are we necessarily observing a chromosphere?

Magnan — The only relevant point is, given a spectrum, can we determine the temperature vs. height relation. Also the definition of an emission line is not clear.

Pecker — The main problem is that we have a hint that emission lines mean something. They might mean many things. The job of the experimentalist is to fish in the pond. The job of the theoretician is to take the fish and see if there is a chromosphere in the fish, or what amounts to a chromosphere.

Thomas — Clearly what you call something is not important. What we would really like to do is to understand what causes the structure of a star. We know the difference between the atmosphere and the interior in a vague sort of way. The only reason one introduces atmospheric subdivisions is because different physical phenomena characterize these different subdivisions. We want really to determine what is the evolution of physical phenomena as I go outward in a star. In a very classical way the temperature and the density, by themselves, will suffice to describe everything, if I can make all of the standard classical assumptions. This isn't true if I go far enough out in an atmosphere. For example, in some cases there is a complete breakdown in the notion of describing a velocity field only in terms of a thermal component and a three dimensional macroscopic component. So, we should make some definition of atmospheric subdivisions which tell you what are the physical phenomena happening in those subdivisions.

Underhill — That is the physical approach and it is a logical and correct one. The problem for the observer is that he normally has to observe over a short wavelength interval. As we extend our wavelength region, we find we are observing different parts of the same object. A physical model which fits well the observations in one wavelength region may not fit observations at all in a different wavelength interval. Trying to extrapolate from one region to another on the basis of physical models is where we go astray. The observers are right to go after emission lines or extra deep absorption lines. In the ultraviolet, however, we have to take care that what we are calling emission lines are not really regions of residual flux between strong absorption lines in heavily blanketed regions. I'm not yet fully convinced of Yoji Kondo's arguments for seeing emission lines, but he really can't say anything else at this stage. With the kind of line blanketing I see at 20 Å resolution in that region in OAO-2 scans, I wonder how much of the "emission" he sees is residual flux between lines. This is precisely the problem — the observations. The more observations we can get the more we're going to know. The theoreticians should proceed, but don't anchor yourselves to a fixed scaffolding of theory and get so fixed that the poor observers think it's there for good.

Kuhi — I don't think the observers have that problem.

Athay — So far I haven't heard two people give the same definition of a chromosphere. Let me be the first to support the definition Larry Auer

gave, namely those situations where the temperatures rise outward in the atmosphere. That is a case where we can hope to give some simple diagnostic to the observer that he can use in saying a certain phenomenon indicates a chromosphere. If the theoreticians want to invent another word to describe a place where there is mechanical energy dissipation, we can leave that up to them.

O. Wilson — I've come to the conclusion after listening to this little hassle, that one man's chromosphere is another man's extended atmosphere. (Laughter and applause.)

Kuhi — I would suggest that we go on now to look at what the observations are trying to tell us. In her survey Francoise Praderie discussed many cases which we can cover one-by-one, starting with the question of excitation anomalies. Were there any questions of clarification about the Ca II H and K lines?

Skumanich — One should be very careful about listing universal criteria for chromospheres, when using the H and K lines. For example, one thing that was listed was intensity — age relationships which only apply to main-sequence stars. As I've shown in a study that has appeared in abstract form only, CaII emission in the K giants is not an indicator of age. There is no kinematic difference, for example, between the emitting and the non-emitting K-giants.

Kuhi — But how about the pre-main-sequence stars? Not the T-Tauris, but those that are farther along than T-Tauris and almost on the main-sequence. Do you know what they do?

Skumanich — No, I don't.

Kuhi — Are there any other questions about the CA II emission in the Sun or in the stars?

Linsky — I would like to show some work by Tom Ayres, Dick Shine and myself at JILA. We have observed a few stars which are reasonably similar to the Sun in an effort to get absolute fluxes if it is at all possible. I'll start by presenting the data on Procyon which is an F5 IV star. Kondo mentioned that there is likely to be emission in Mg II H and K in this star. What I have here in Figure II-25 is a low spectral resolution scan of the region including Ca II H and K. The units here are flux in $\text{ergs/cm}^2/\text{sec}/\text{Hz}$ at the surface of the star. I show this scan for two reasons: (1) to show that at low spectral resolution you see no emission in H or K and (2) to show how we calibrated our data in absolute units at the surface of the star. We took a 10 Å interval centered at 3950 Å and tied this through photometry to Vega at 5000 Å for which an absolute flux is known. We put in the radius and parallax of the star to

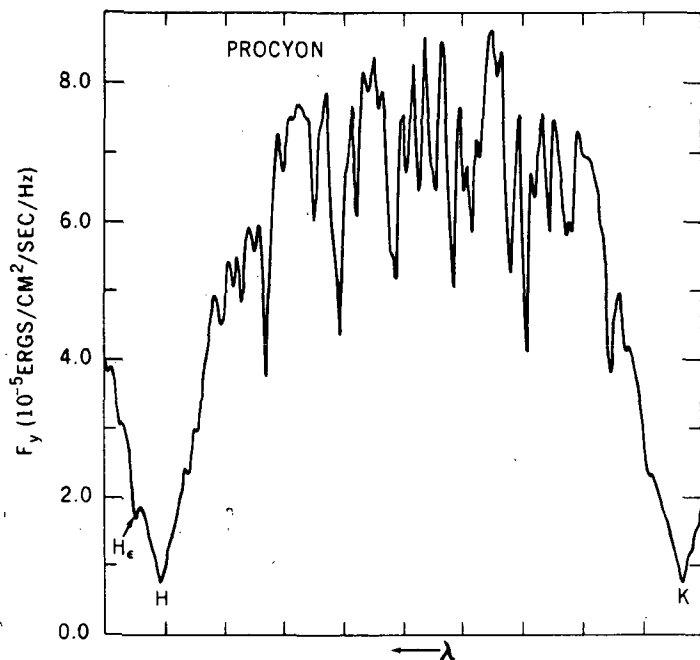


Figure II-25

get absolute values of the flux at the surface of the star. Figure II-26 shows a high resolution 7th order scan of the K line of Procyon. This is in the center of the line over about 1.7 Å interval. The data have been filtered. This is data taken with the Kitt Peak solar tower and I might point out that this represents 5 hours observing. Again the units are flux at the surface of the star. On the right hand side of the diagram we have turned the flux units into an equivalent brightness temperature. In this scan we see a profile very similar to the K line in the Sun. There is definitely a reversal on the violet side, although such a reversal is unlikely on the red side. Also the brightness temperature corresponding to K_1 is about 4900° . If the minimum temperature in the Sun is about 4300° , which is the same as the brightness temperature in K_1 , and if one thinks of K_1 as a good measure of the minimum temperature in the Sun, then we may indeed have a direct measure of the minimum temperature in Procyon. What is especially interesting is that the ratio of the brightness temperature in K_1 to the effective temperature of the star is 0.745 for Procyon and the Sun.

It may well be that there is a scaling law which is applicable, wherein the physical processes that determine the minimum temperature in Procyon and the Sun are the same. So perhaps one could extrapolate at least over

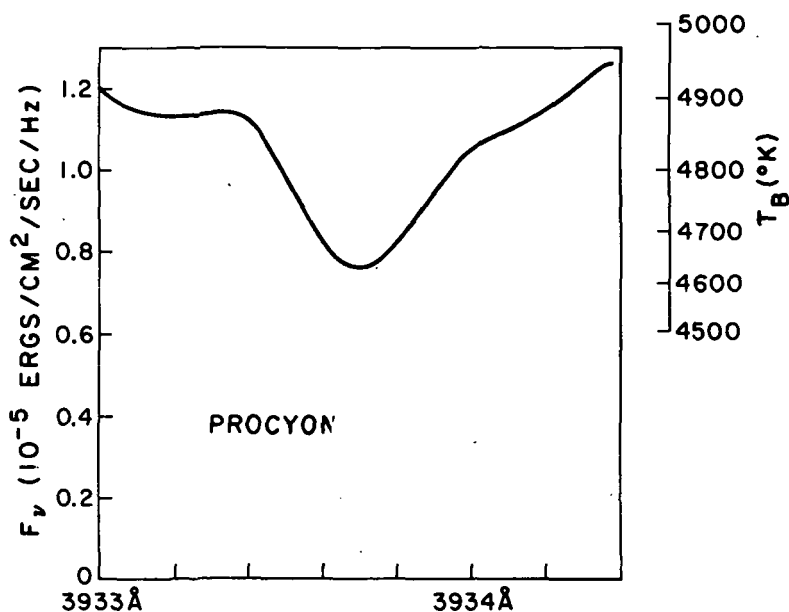


Figure II-26

a limited range in the H R diagram to determine the minimum temperatures in related stars.

Figure II-27 shows the K line profile of Procyon again, now in residual intensity units. Also shown is the K line for the Sun, now viewed as a point source. This is to show that the shape of the profiles is the same, although the Procyon profile is much broader. In addition the K line for Arcturus is shown; it possesses a much more significant double reversal. The data for Arcturus are taken from Griffin's Atlas.

Figure II-28 shows additional data we obtained for Procyon near 8542 Å (the pluses in the diagram). Note the central intensity in $\lambda 8542$ is the same for Procyon as for the integrated solar flux, although the Procyon profile is of course broader. We also observed Aldebaran (K5 III) where the profile is actually quite similar to the solar core.

Peytremann – How did you put the Sun on a flux scale?

Linsky – We put the Sun on a flux scale by taking the observations at the center of the disc and at a few μ points and doing an integration. We also took into account continuum limb darkening. It is sort of a fictitious, quiet Sun as we've ignored plages, active regions, etc.

In Figure II-29, if again we go to Griffin's Atlas for Arcturus and plot the five CaII lines on the same scale with residual intensity on the ordinate

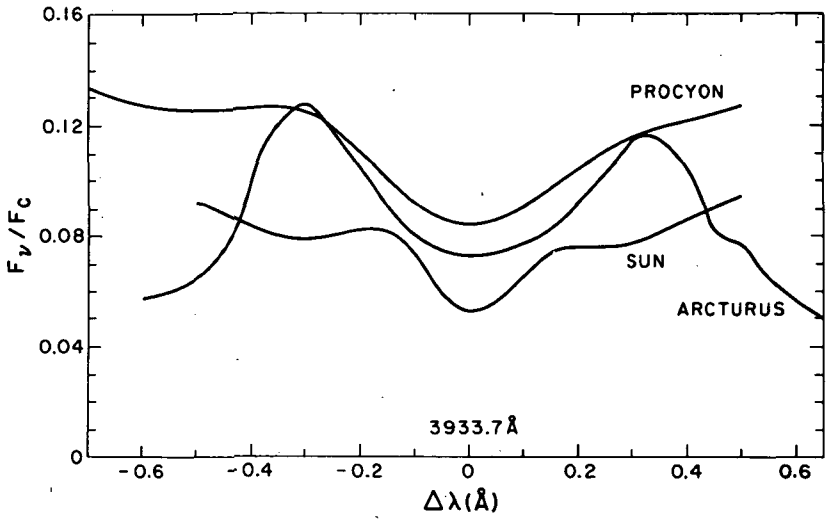


Figure II-27

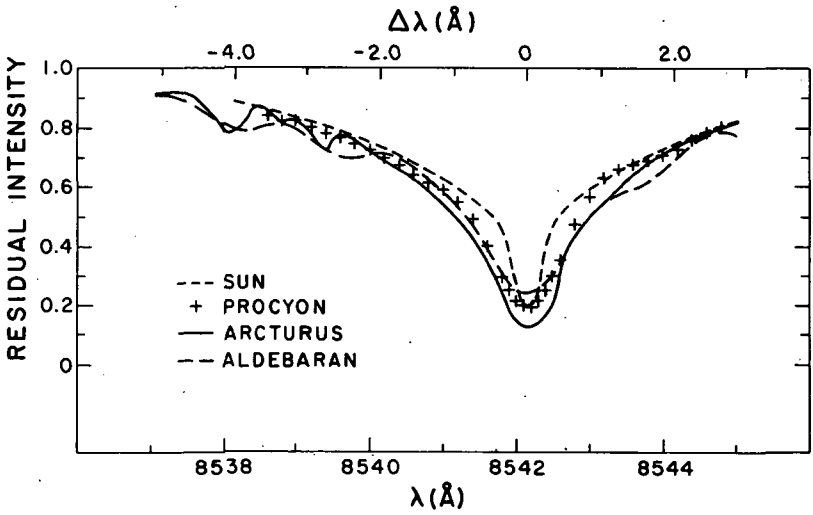


Figure II-28

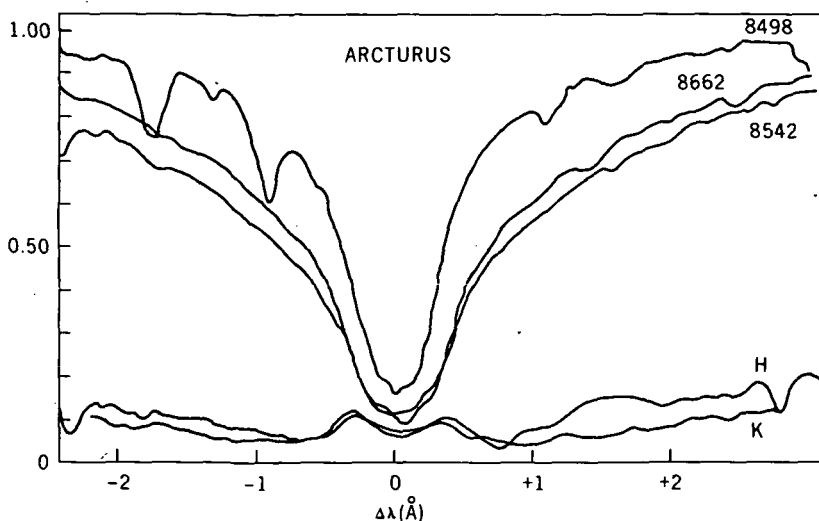


Figure II-29

and a common $\Delta\lambda$ scale on the abscissa, two things strike me as interesting. You'll recall that yesterday I showed observations of a very weak solar plage in the same five CaII lines. There is quite a lot of similarity between that case and Arcturus. In a very weak plage in the Sun you get some emission in H and K (of course it is broader in Arcturus) and you get pure absorption lines in $\lambda 8542$ and $\lambda 8662$. In the weakest of the triplet lines, $\lambda 8498$, there is a hint of a central emission, in the plage. There is also a hint of an emission feature in $\lambda 8498$ in Arcturus, as taken from Griffin's Atlas. It may well be that $\lambda 8498$ is a very interesting line to look at in a range of stars, as an indicator of chromospheric emission.

Athay — Jeff, is it certain that $\lambda 8498$ does not have a blend in there?

Linsky — There are no known lines at the required wavelengths. It would have to be a very complicated blend, being the same in Arcturus as in plages, but absent in the quiet Sun.

In Figure II-30 we have a low resolution scan of Aldebaran (K5 III), taken at Kitt Peak. The point here is that even at low resolution (20,000-30,000), you can see emission in the core of H and in the core of K. The emission is brighter in K than in H. The low resolution eliminates the K_3 feature.

In Figure II-31 we have a low resolution scan of Sirius which shows that CaII H and K exist in this star and that H is a small perturbation in the wing of He.

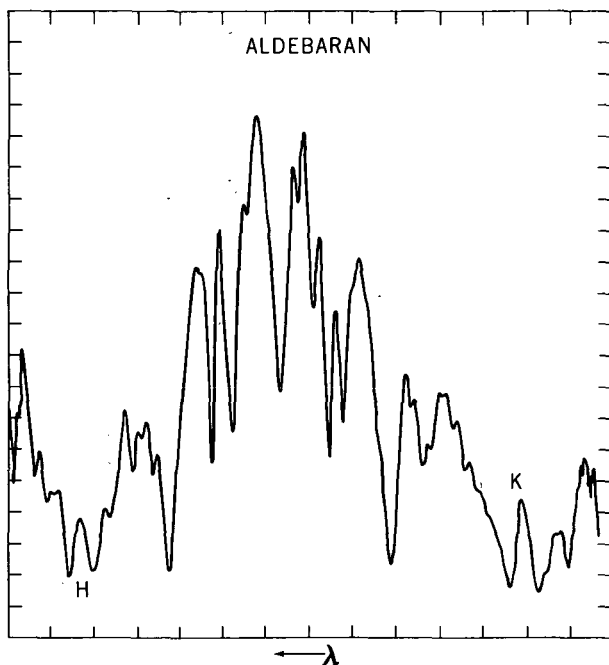


Figure II-30

Figure II-32 shows a high resolution scan of Vega. This is really quite interesting. Here we are in the broad wing of H_ϵ with the wing decreasing in this direction. This is about 10-15 minutes worth of data taken while we were waiting for Procyon to rise. We didn't really expect to see very much in Vega, but it may well be that this feature seen on the red wing of the H line of CaII is in fact an emission feature. This may indicate a chromosphere on a star as early as an AO dwarf.

Praderie — What is the wavelength scale?

Linsky — It is about 1.4 or 1.7 Å for the full width. Before anyone takes this too seriously, I should mention the last figure, Figure II-33. This illustrates the unfiltered data, for purposes of honesty. This is the emission feature I was talking about. The data are very noisy and the observations should be done again. The emission hump does seem to be there in the unfiltered data and if you look then at the filtered data, perhaps the hump is there or perhaps it is not. I wouldn't stake my life on it. However, I wouldn't be surprised if Vega, which has already been mentioned as a star potentially with a chromosphere, indeed shows some emission in the CaII H line.

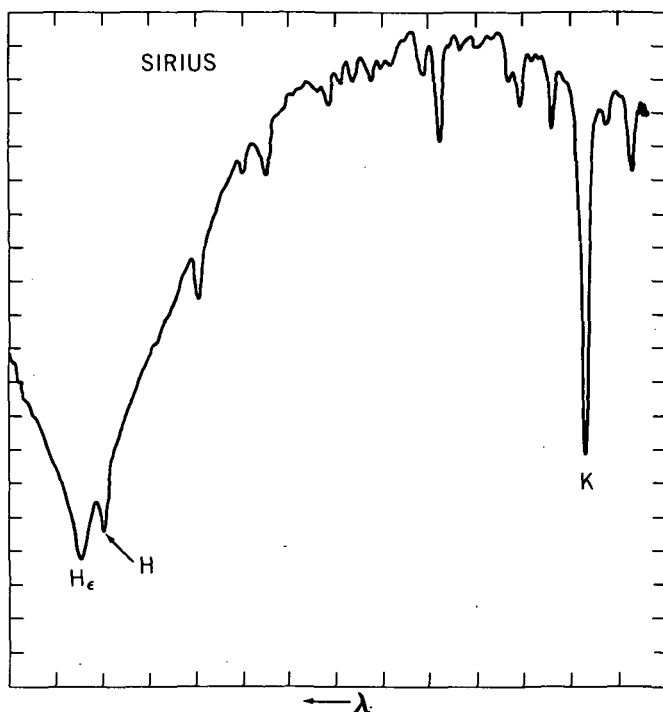


Figure II-31

Underhill — Might the CaII K line for Vega be double?

Linsky — We had intended to do both the H and K lines on Vega after we had seen data of this sort. However, it snows on Kitt Peak. We'll have to wait for our next observing program.

Kuhi — The next major topic covered by Francoise was also related to CaII emission, namely the Wilson-Bappu effect. I have one question about this. It is always stated in the literature that a correlation exists between the absolute visual magnitudes and the width of the CaII K emission. Has anybody looked to see if there is a correlation with absolute bolometric magnitude as well since the bolometric corrections are so small for these stars.

O. Wilson — I've never done that. I presume that there is a correlation, but it wouldn't be linear. I do not know what the correlation is. I've always used the visual because there the correlation is beautifully linear and therefore handy.

Peytremann — I have some comments about the Wilson Bappu effect. Yesterday, Gene Avrett told you about some theoretical non-LTE com-

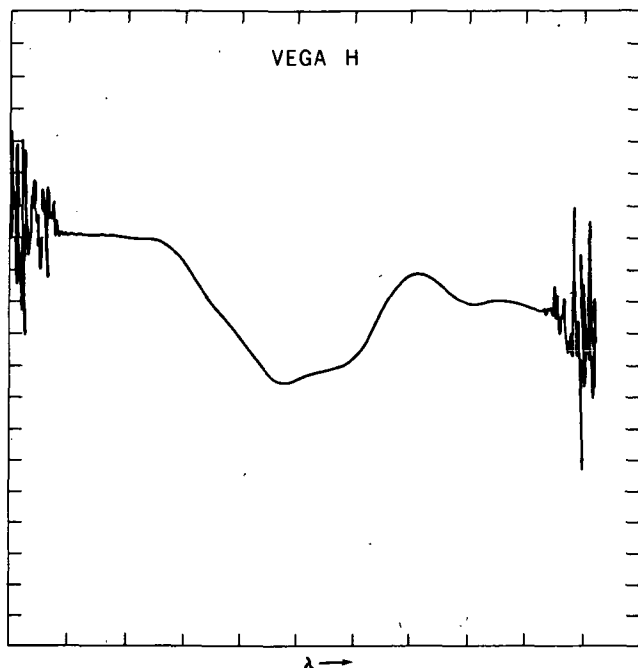


Figure II-32

putations we have done at Harvard on calcium line profiles. He showed some profiles which I will not show again now. Once we had these profiles, we tried to test them against one very well established observed effect, i.e., the Wilson-Bappu effect. The first question that arises is again one of definition, but this time it is a definition related to the observed quantity. The width of the CaII K emission as defined by Wilson and Bappu (1957) is the difference in wavelength between what they call the violet edge and the red edge of the emission. If you have a theoretical profile you also need to define "an edge." On the top of Figure II-34, I show the red part of a calcium line with a flux scale on the ordinate and arbitrary wavelength units on the abscissa. I adopted three possible definitions of the width, which I call W_1 , W_2 , and W_3 . W_1 is the width at the minimum, K_1 . W_2 is the width at half the flux difference between the maximum of the emission, K_2 , and the minimum, K_1 . W_3 is the width at one quarter the height in flux units between the maximum and the minimum. This is important as will be seen in Figure II-35. I should add that if you measure the width on a photographic plate, even if you have the densitometry profile on the plate, you still are on a density scale. Even if you define the width on a density scale on the photographic plate, you still have to convert it back to flux units before

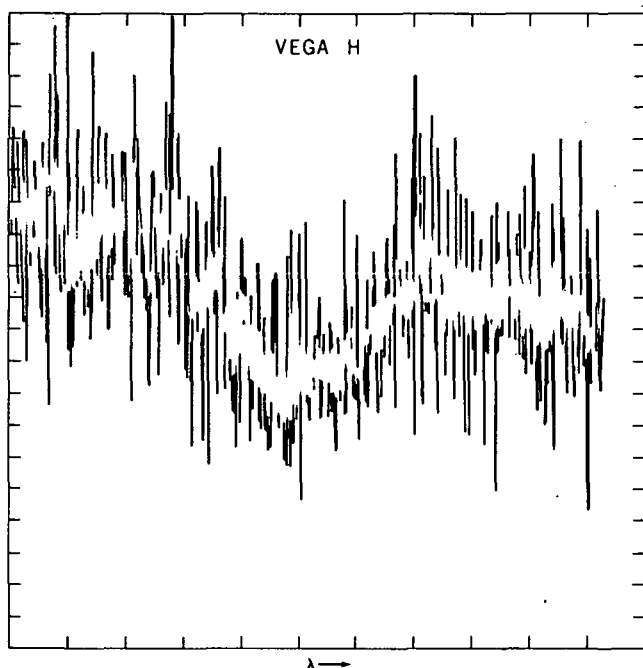


Figure II-33

comparing it to theoretical calculations. Obviously, a densitometry profile is not going to look the same as a flux profile.

In Figure II-35, I plotted absolute magnitudes as a function of the log of the half-width as defined by Wilson and Bappu. Before I discuss this graph, I have to say how we go from model atmosphere calculations to an absolute magnitude scale. The absolute magnitude is

$$M_v = -10 \log_{10} T_{\text{eff}} + 2.5 \log_{10} g - 2.5 \log_{10} M + C_{\text{bol}} + \text{constant}$$

M_v = absolute visual magnitude

T_{eff} = effective temperature

g = surface gravity

M = stellar mass

C_{bol} = bolometric correction

In model atmosphere computations I specify T_{eff} and $\log g$ and also roughly the abundance — metal poor or metal rich. These three quantities

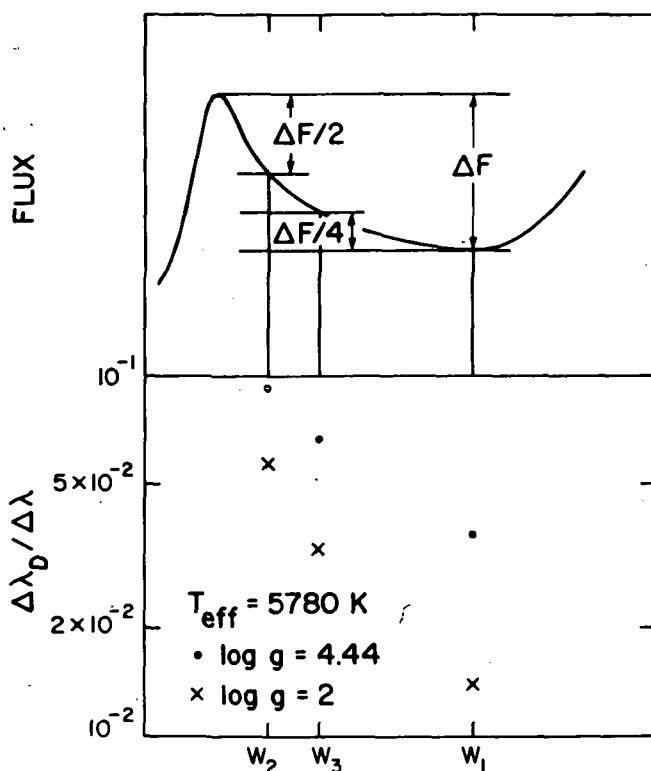


Figure II-34

do not allow me to uniquely define the absolute magnitude, because I need the mass. I don't know anything about the mass in atmospheres that are roughly plane parallel. The bolometric corrections can be taken from metal line blanketed models and in any event this correction is not too big in the range between $T_{\text{eff}} = 4000^\circ$ and 6000° K. The main problem is how do we get the mass. We can start from evolutionary tracks in terms of gravity and T_{eff} ; i.e., one looks at that star which at some point in its evolution would have a specified T_{eff} and $\log g$. This star has a certain mass, which one uses to calculate M_v . Here we have to rely on evolutionary model calculations and that introduces another uncertainty. This solution is not necessarily unique because there can be a region in the HR diagram, corresponding to a $T_{\text{eff}} - \log g$ combination through which stars of different masses can evolve. That is an uncertainty that can bring trouble.

We started with a solar model. We then calculated another model in which we just changed one parameter — i.e., the surface gravity — and left everything else as in the solar model. Avrett described yesterday how

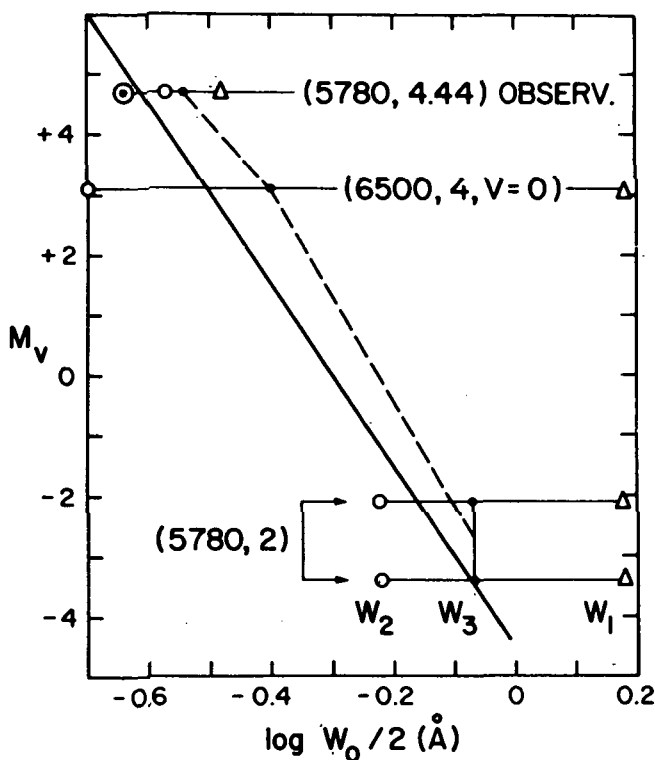


Figure II-35

we re-scaled the temperature. There will be objections to the way we did this rescaling in order to have a chromospheric rise. For what we want to show, this is not an important problem. We just want a temperature rise in order to get an emission peak in calcium. It has been shown that the Wilson-Bappu effect is independent of the intensity of the emission peak. So whatever temperature gradient we take should give the right answer as far as the Wilson-Bappu effect is concerned. What we then have to prove is that it is also going to work for temperature gradients other than the ones we have adopted.

On this graph I show the Wilson-Bappu relationship as a solid line. The value for the sun given by Wilson and Bappu is indicated by \odot . The open circle (o) corresponds to definition W_2 , at half-height between K_2 and K_1 . W_1 (Δ) is at K_1 . W_3 (\bullet) is in between. The first thing that you can see is that the results one obtains depend significantly on which width definition one adopts. For the Sun the problem is not too bad, but for the giant ($\log g = 2$) case, the definition adopted can change very significantly the results you get for the theoretical width. An extreme case is the

model at $T_{\text{eff}} = 6500^\circ\text{K}$ and $\log g = 4$, where the emission peak is very narrow. (We have taken zero turbulent velocity in this case.) Then one has a very flat K_1 minimum. In such a case, one is in trouble because there is a tremendous difference depending on whether one adopts the definition W_3 or W_2 , W_1 being obviously inappropriate.

In addition to the solid line, I have shown a dashed line which joins the points corresponding to definition W_3 . In this case, the slope is roughly parallel to the observed effect, although there is a slight shift to the right. However, if one takes the giant ($T_{\text{eff}} = 5780^\circ\text{K}$, $\log g = 2$) case, one sees that the calculated points between W_2 and W_3 bracket the observed relation.

For the model with $T_{\text{eff}} = 5780^\circ$ $\log g = 2$, and from evolutionary tracks (Iben, 1967) I derived a mass of $6M_\odot$, which corresponds to $M_v = 3.4$. In addition to this procedure I took a more direct route to get M_v . In a recent paper by Bohm-Vitense (1971), a star of luminosity class II has $\log g = 2$. With this and the spectral type one can go to tables like the one of Schmidt-Kaler (1965), which then gives $M_v = -2.1$. This gives two independent determinations of M_v . One sees that the observed width - luminosity relationship (solid line) is bracketed by the theoretical joints between definitions W_2 and W_3 , and $M_v = 3.4$ and -1.4 . Within the uncertainties in the width definition and in the derived values of M_v , it would seem that we can explain the Wilson-Bappu relationship just in terms of an opacity effect. We did not put in any extra velocity fields. I do not say that there are no velocity fields. But such fields may not be required to explain the Wilson-Bappu effect. These are very preliminary results which are presented here only because this meeting is supposed to be a working conference. Further calculations with various temperature-height relations are needed to confirm these first results, and to improve the shape of the emission peak. These investigations are currently under way.

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CONTINUATION OF DISCUSSIONS FOLLOWING TALKS BY PRADERIE AND DOHERTY

Kuhi — Peytremann has given us a very interesting explanation of the Wilson-Bappu effect which did not require the velocity parameter suggested by others and relied entirely on the opacities. I wonder if there is any comment or discussion on this point.

Rosendhal — It should be pointed out that there is some observational evidence that velocity fields may have something to do with the Wilson-Bappu effect and other related phenomena. Referring to observational studies in the literature, in the case of the F stars, Osmer has empirically established that there is a correlation between the width of the infrared oxygen lines at 7774 and absolute magnitude. There is nearly a linear relationship for stars more luminous than absolute magnitude -2 or -3. He also finds that in this absolute magnitude range a change in the behavior of the turbulent velocity in the sense of an increase in the more luminous F stars, and that you can completely explain the dependence of the width of the infrared oxygen lines from the increase in turbulence in these stars. The second point which I think is important is that a couple of years ago a paper appeared by Bonsack and Culver who looked at the line widths and turbulence in the K stars. This was prompted by Kraft's observations of H β as an indicator of absolute magnitude through an analogous effect to the Wilson-Bappu effect. They also found that there was a correlation of turbulence as derived from the curve of growth with the width of H β . Therefore in two cases, namely that of the K stars and also the highly luminous F stars, there is some empirical evidence that velocities are relevant to the problem and that there is a relationship between the observed velocities and various types of luminosity indicators.

Peytremann — Many people who have tried to interpret the Wilson-Bappu effect in terms of velocities have thought that the widths represent velocity broadening in a direct sense and did not base their analyses on any sort of detailed model calculations to make sure that the broadening did not come about indirectly through some other intermediate mechanism. You mention H α profiles, and I ask how you know that what may seem to be velocity broadened widths are *really* velocity effects.

Rosendhal — I didn't say H α was broadened by velocities. I merely pointed out that the observed changes in the width of H α are correlated with something which is associated directly with a velocity parameter, and that H α exhibits a behavior analogous to the Wilson-Bappu effect.

Kippenhahn — The fact that a stellar atmosphere doesn't know about the mass but only about effective temperature and gravity has been a basic difficulty with the Wilson-Bappu effect. The situation is very similar in a

quite different field in astrophysics, namely, in the explanation of the period-luminosity relationship of the Cepheids. There, as well as here, one needs information about the mass of the stars in a given region of the HR diagram, information which can only be obtained from evolution theory. Evolutionary tracks project the mass-luminosity relationship from the main sequence into the region of the evolved star, and, although there is some scatter, this procedure brought out the explanation of the mass luminosity relationship (Hofmeister, Kippenhahn, Weigert, 1964, *Zeitschrift f. Astrophys.* 60, 57; Hofmeister, 1967, *Zeitschrift f. Astrophys.*, 65, 194). What Dr. Peytremann suggested this morning is very similar. When he assumed that for the red giant region stars of a given luminosity have a certain mass he assumed that there is a mass-luminosity relationship for evolved stars (which is not the classical mass-luminosity relationship for main sequence stars).

I wonder whether one would not get a similar phenomenon for the width-luminosity relationship as one encountered already for the period-luminosity relationship. In the case of Cepheids we know that stars which have undergone a different evolution like the W Vir stars (whose evolutionary history is still unknown) have a different mass-luminosity relationship when they cross the Cepheid strip and therefore have a different period-luminosity relationship. Similarly in the case of stars with CaII emission: if another population of stars is observed in a certain area of the HR diagram they might have masses different from that of population I stars in the same region of the diagram. Should they not show a different Wilson-Bappu relationship? Can one look for this, or is the effect of different masses obscured by the effects due to different metal content?

Athay — There is, I think, an observational way of deciding whether the emission extends into the damping wings or is due to a velocity parameter. When Skumanich and I looked at the problem several years ago we found the same effects that Peytremann has described, but they implied that the line wing is producing the broadening, and that there is a correlation between the flux in the K emission and the width of the emission peak. If you increase the opacity in the chromosphere, that both broadens the peak and increases its flux, and I don't see how you can avoid that, at least for stars of the same age. Only if you deal with stars of different ages would you be able to destroy the correlation.

Peytremann — I agree that this correlation should not exist for stars of the same age, and, indeed, this point will be investigated.

Jefferies – I think that in fact the answer may be with us already from some observations that were shown this morning. There are two things that determine the separation of the peaks used in the Wilson-Bappu relationship.

One is the Doppler width and the other is the optical thickness of the chromosphere. We should be able to differentiate between these two by using profiles of the H and K lines of ionized calcium and magnesium. Since these will have the same Doppler (velocity) widths, while the optical depths of the chromosphere in the two sets of lines will differ in proportion to the relative abundances, I think, therefore, that one should be able to determine the major contributor to the width from using a little theory and making a comparison of Wilson-Bappu relationships for the calcium and magnesium lines.

Kuhi – The Mg II relationship does seem to have a flatter slope but is based on only a few points.

Linski – An interesting result comes from looking at solar plages concerning the Wilson-Bappu relationship. Consider the relation between the K line width, determined say at the half intensity point between K_2 and K_1 and the activity of the plage, both the width and intensity increase. From a weak plage to a strong plage, the width does not increase while K_2 does increase. I think the physical explanation of why this happens in the Sun would be of great importance in understanding the Wilson-Bappu effect.

Wilson – I would like to ask Jefferies a question about the Ca and Mg Magnitude – width relationship he discussed. If you look at two stars with the same luminosity but a different calcium abundance, presumably, you won't get the same results.

Jefferies – I can't offhand answer the question of what happens with different abundances, particularly with a different Ca to Mg abundance ratio from star to star.

Wilson – If you have one group of stars with a solar Ca abundance and another series of stars with, say, only one fifth that much Ca, would you expect to get two different magnitude-width relationships?

Jefferies – To the extent that the position of the bottom of the chromosphere isn't dependent on the Ca abundance that may be the case. Such a result may seem implausible, but so is the Wilson-Bappu relationship.

Wilson – There are many comments in the literature, as you know, about possible abundance effects but I think the evidence against such explana-

tions is quite strong. I will have more to say on this in my talk at the end of the meeting.

Pasachoff — I have suggested in an *Astrophysical Journal* paper (164, 385, 1971) one more thing that helps explain the Wilson-Bappu effect in the Sun. The Sun is, after all, the star in which one can study how the actual line profile that we measure is constructed. If we look at Figure II-36, we

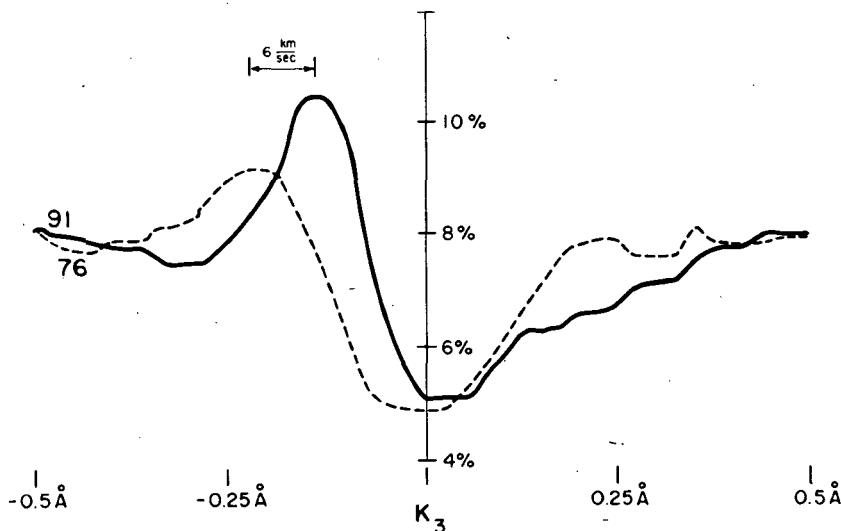


Figure II-36

see profiles of two fine structure elements located about a second of arc apart from each other. One can see that each profile for the K line is very different from the profile for a neighboring element. These are what the supposedly symmetric double-peaked profiles look like under high spatial resolution. The K_2 peaks on the violet side of these two profiles appear in two rather different locations, a few hundredths of an Angstrom from each other. The statistics of how these peaks vary show that there is a contribution of several km/sec to the line width of the Sun. Similar contributions must also arise in the other stars we see.

Magnan — I think that the turbulent velocity is only a parameter that is put into the calculation for convenience. I think that the best indication for velocity fields comes from the asymmetry of the line. I think it is important to account for different intensities in the red and blue wings.

Kandel — I think that from the diagram that Pasachoff showed, the velocity differences in the separate cases would be assigned to macro-turbulence.

Pasachoff — We all agree that the reason the averaged peaks have their observed separation and are asymmetric is still controversial. The simple models that just have Doppler shifts one way and the other can certainly be challenged on many grounds. While the peak displacements can be represented on a velocity scale, it is not necessarily the case that there are elements moving at these velocities.

Jennings — Praderie mentioned in her talk a correlation which has been published concerning calcium H and K emission, infrared excesses and polarization. Since the initial report quite a bit of work has been done on this at Kitt Peak and we have some results which differ from those which are in print. We have considered H and K, hydrogen, Fe II and other emission lines in late type giants and supergiants and we find the following results. If we plot the mean change in polarization vs. the ratio of intensity in the K line to that in the continuum we find that the stars break very neatly into two groups. Those that are intrinsically polarized show no Ca emission detectable at Wilson-Bappu intensity class II or greater. On the other hand, stars which do not show intrinsic polarization do show very strong Ca reversals. There is one star which tends to bridge this gap, α Ori. This star shows very weak polarization, and, as you know, Ca reversals.

Further, Dyck and others have discussed a correlation between polarization and infrared excesses, so we can also add infrared excesses to the graph. Combining these two pieces of data we interpret this to mean that those stars which are surrounded by circumstellar material do not tend to show Ca H and K emission. We have also looked at other emission lines, notably Fe II and we find that the result holds for these lines; i.e., stars with infrared excesses and intrinsic polarization do not show Fe II in emission. The only star which does is α Ori. But again this is a case having very weak polarization, and very weak infrared excesses. It should be noted that this particular relationship conflicts with that originally mentioned by Geisel who suggested that the presence of Fe II is accompanied by infrared excesses. We find this not to be the case. It is also interesting to note that among the stars which do not show polarization none currently show hydrogen emission nor have we been able to find any reference in the literature to hydrogen emission among these objects. On the other hand, approximately 50% of those stars which are high polarization objects have shown or are showing hydrogen emission. Also, this is the strange type of emission which is shown by Mira variables, i.e., having a distinctly anomalous decrement. Two cases of this type presently in emission are Z Ursa Majoris and RX Boo where we find that H α and H β are missing, H γ is weakly in emission, H δ strongly in emission, H ϵ is missing, and H8 through H10 are weakly in emission. The explanation for this seems to be that these lines arise far down in the

photosphere, and are affected by strong overlying absorption. This is the current status of the emission line vs. grain indicator correlation.

Kuhi — In defense of Susan Geisel's comments, I think that in her paper she certainly did not mean to imply that 100% of stars that showed Fe II emission had infrared excesses. I think her batting average was around 80%.

Jennings — Among the late type stars the correlation seems to be exactly the opposite. If you find Fe II emission you do not find infrared excess.

Pecker — Your measurements all refer to rather cool stars, those showing the K line, and Susan Geisel picked primarily Be stars. For Be stars do you still find the correlation between Fe II emission and the absence of infrared excess?

Jennings — I meant only to say that Susan Geisel's correlation is reversed in the case of late type stars. Her correlation seems perfectly valid for stars of early spectral type.

Boesgaard — What data do you have for the Fe II emission lines for late-type stars and how many stars did you observe?

Jennings — Of thirty stars or so, seven or eight showed strong polarization and for these we found no iron emission and none seems to be reported in the literature. Fe II emission is fairly common among those stars which don't show polarization.

Leash — We do not seem to have directly observable indicators of the chromospheres in early type stars. I wonder if Praderie has any opinion on whether the lines of Si II at 4128 Å and 4130 Å might be a good indicator of chromospheres in B stars?

Praderie — I have tried to determine the dominant terms in the source function for the Si II resonance multiplet at 1808, 1817 Å in A and B stars. The source function is collision dominated. I don't know the situation for Si II 4128 Å and 4130 Å, and have not considered B stars.

Heap — I would like to suggest the O stars as candidates for having chromospheres on the basis of observations by Slettebak in the 1950's. Slettebak measured the broadening of lines in O-type spectra and found that there was no O star whose spectrum shows lines sharper than about 75 km/sec. His sample was large enough that he should have been seeing some of these stars pole-on. He concluded that there was some intrinsic velocity broadening, eg. turbulence, present in early O stars. Also, Aller's plates of planetary nuclei having O or Of-type spectra show at least 75 km/sec broadening. Hence, there are no O stars, young or old, that have sharp lines. This is a serious problem because of velocity of 75 km/sec is about twice the speed of sound in the atmospheres of hot stars.

Underhill — For the O stars there is no difficulty in explaining the hydrogen line widths at least, but you are correct in stating that sharp lines are not seen in O star spectra.

Kuhi — Also we must consider the problem of radiation pressure in these very hot stars, which may be very efficient in forcing material away from the star. This could prevent the formation of a chromosphere.

Boesgaard — I wish to report on the ultraviolet Fe II emission line in α Orionis. It is perhaps too bad to leave the Ca II emission line which is the one thing everyone seems to agree on that indicates the presence of a chromosphere. Inasmuch as Francoise Praderie implied that the Fe II emission lines may be formed in a circumstellar shell, when I talk about these Fe II lines I should adopt Olin Wilson's feeling about a chromosphere: one woman's chromosphere may be another woman's extended atmosphere. In any case α Ori offers ample proof of both a chromosphere and an extended envelope. It does show the calcium emission and it certainly shows blue-shifted circumstellar cores in zero-volt absorption lines. These Fe lines were first discovered in 1948 by Herzberg (Ap. J. 107, 94). There are about 17 observable lines from multiplets 1, 6, and 7 of Fe II. These lines occur in the region 3150 Å to 3300 Å which makes it very difficult to look for them in cool stars since they radiate so little energy that far in the ultraviolet. About the best candidates are α Sco and α Ori and even these require long exposure times for high dispersion studies. Bidelman and Pyper (1963 P.A.S.P. 75, 389) looked at something like 6 M stars, one MS star and a carbon star for these lines.

Figure II-37 shows an ultraviolet spectrum of α Ori at 3.3 Å/mm taken at the Mauna Kea Observatory 225-cm telescope. The iron emission lines are indicated there. Of those 17 lines about 7 are so badly mutilated by some kind of overlying absorption that little can be learned from them. (A figure in Doherty's talk showed profiles of two Fe II lines: one with a strong self-reversal and a second line which has a high laboratory intensity but which is too mutilated to give any radial velocity information.) The feature at 3228 Å looks like a double emission line but is actually a strong emission line with a central absorption reversal. The line at 3277 Å is an example of a strong emission line with a weak self-reversal. The lines in the region around 3167 Å are among the weakest lines with no reversals.

I measured the radial velocities on four separate spectrograms taken over a period of a year from November 1970 to December 1971. The absorption lines give a radial velocity for the photosphere. α Ori is known to have a variable radial velocity as the photosphere seems to be pulsating. The velocity there is about 21-22 km/sec and shows a range of about 4 km/sec. Measurements were made to determine radial velocities of the absorption lines, the emission lines, and the self reversals; the

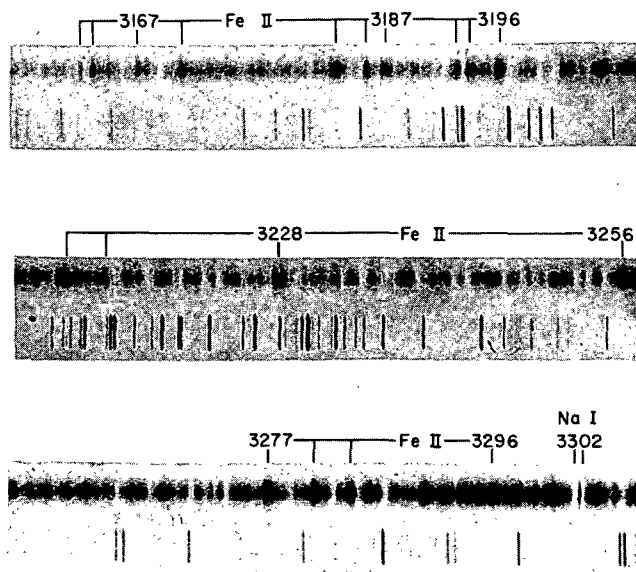


Figure II-37

results are shown in Figure II-38. The first panel shows radial velocity measurements and probable errors on 4 plates for the absorption lines. This variation is what is expected for α Ori for the photosphere; it shows a range of a total of 4 km/sec. The average velocity is about 22 km/sec. The next part of Figure II-38 shows the velocities for the emission lines. The large dots at the top are from the seven strongest emission lines in the spectrum; this looks like the velocity is constant for those emission lines. *The region where the emission lines are formed does not take part in the photospheric variations.* The four small dots below that are the radial velocities of three weak lines. The probable errors are similar to those for the strong emission lines but are not shown for the sake of clarity. The third part in Figure II-38 shows the positions of the reversals.

Now if you have looked at the scale on the left you may be perturbed by the fact that these emission lines show a red-shift. That usually indicates *infalling* material. If the chromosphere or envelope is expanding, I find it difficult to understand such a shift, but Grant Athay has assured me that it is possible, even in an expanding atmosphere, to get red-shifted emission lines. If you look at the average of these velocities, the emission lines are red-shifted by about 5 km/sec relative to the photospheric lines. The reversals, except in the one case of KE-33, are slightly blue-shifted within the emission features. That we can understand as cooler material farther out in this expanding atmosphere. So the reversals are about 3

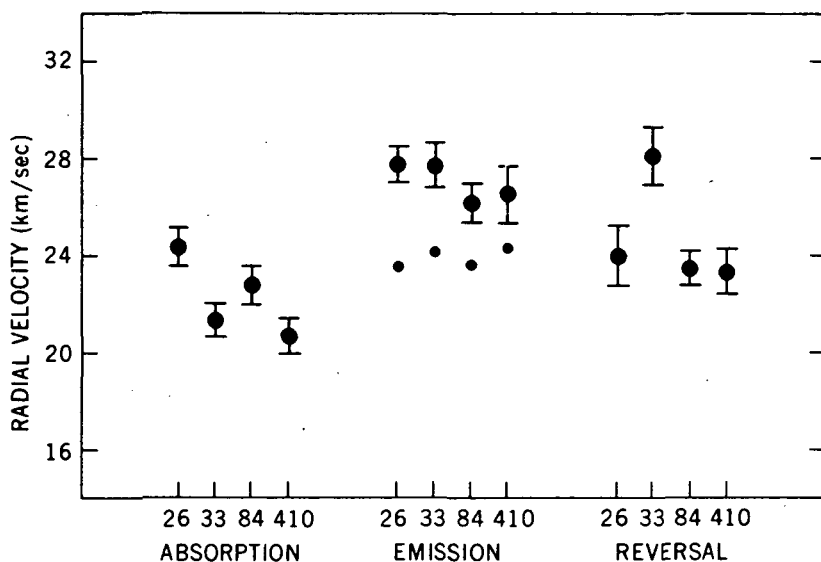


Figure II-38

km/sec to the red of the photospheric lines or about 2 km/sec to the blue of the emission lines. Incidentally, in the same star Olin Wilson long ago measured the velocities for the calcium emission and the K2 features are also shifted to the red by 4 km/sec.

Determinations of relative intensities, half-widths, and intensities of the reversals have also been made. For Figure II-39 I have averaged emission intensities that are eye estimates on the four plates that I have and plotted them against half-width, that is, width at half intensity. There is a linear correlation between the intensity and the breadth of the line. The scale shown on the right in the figure is in km/sec; the weakest lines are about 20 km/sec in width and the strongest line has a width of about 85 km/sec. Figure II-40 shows the relationship with the reversal intensities. Not all the lines are self-reversed; those are the weak ones and the reversal intensity is zero. The medium intensity lines have medium reversals and the strong line at 3228 Å has a very strong central reversal. This figure is again the *average* intensity from the 4 spectrograms. There are plate-to-plate variations so reversal intensities for medium-strength lines range between 1 and 3, but none are ever called 4. For individual spectra these diagrams show linear correlations without the discontinuities seen in this averaged diagram. If we look again at the km/sec scale for the widths, the unreversed lines have an average width of about 30 km/sec. The middle ones have widths of about 60 km/sec and there is the one strong one at

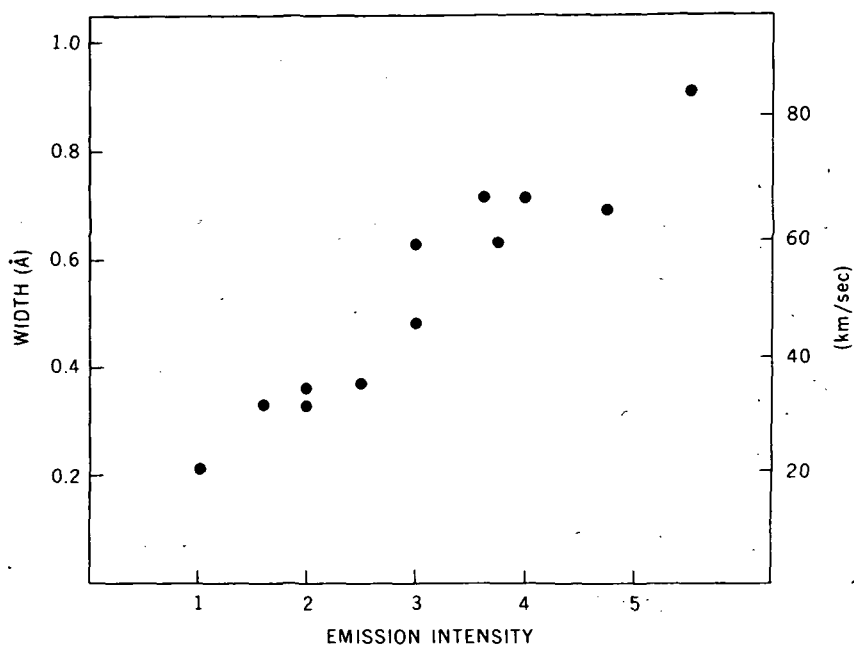


Figure II-39

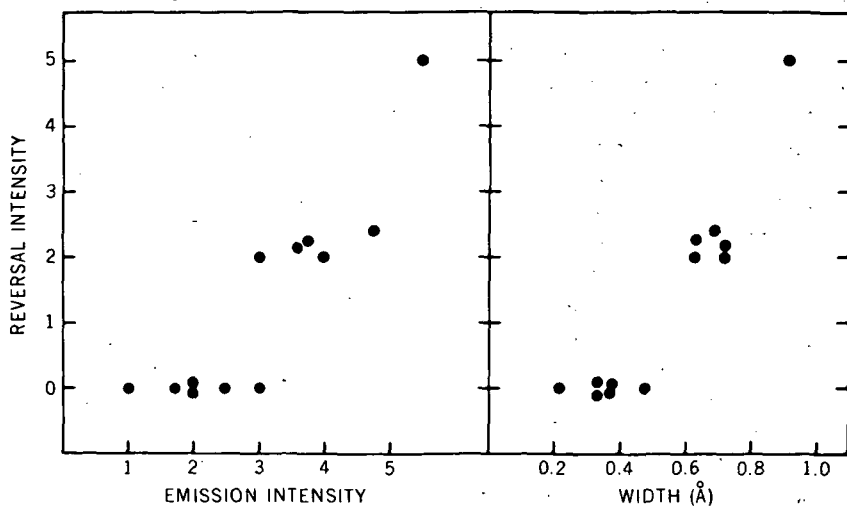


Figure II-40

85 km/sec. The width, W_0 , measured by Wilson for the ionized Calcium line is 170 km/sec.

Figure II-41 depicts profiles of some of the lines. The first one, 3166.7 Å is an example of a weak line; 3196.1 is one of the medium strength lines

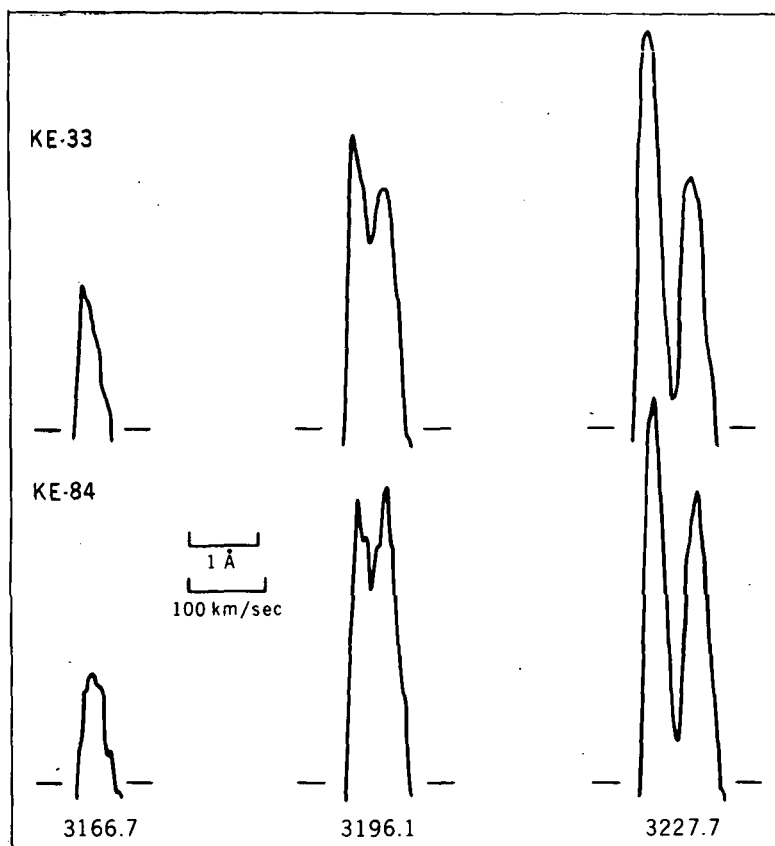


Figure II-41

with a self-reversal. Also shown is a strong line, 3227.7 Å, with a strong self-reversal. The upper and lower set of profiles are from two different plates taken several months apart. For the self-reversed lines on KE-33 the blue peaks are stronger than the red peaks, and the weak line is asymmetric. There is some variation with time in the exact structure of the iron emission lines in this star. All the lines in KE-84 seem more symmetric like the examples in Figure II-41.

The time variation for the Ca line structure is shown in Figure II-42. The solid line is from KE-33 taken on November 14, 1970, while the dotted

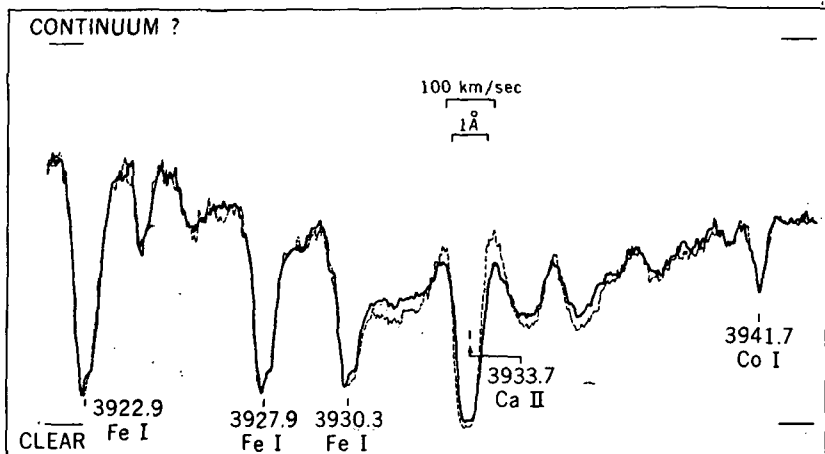


Figure II-42

line is from KE-410 on December 6, 1971. There is a very broad, shallow K_1 and H_1 in this star. A continuum point about 17 Å to the ultraviolet from this line is indicated at the top of the figure; the actual continuum is probably higher. The line-center position shows that the K_3 core is slightly blue-shifted. The K_2 emission peaks show on either side of K_3 . Wilson's data give a slight red-shift for the emission peaks, +4 km/sec. You can see that there are some variations in the Ca intensities and in the K_2 blue-to-red relative strengths. For KE-410 note that the red peak is stronger than the blue.

There is a large amount of information available about chromospheres in the iron lines. There are many lines for one thing, at least 10 that are particularly useful and about 17 that give some information. For α Ori the photospheric lines show the expected velocity variation while the constant-velocity Fe II emission lines are red-shifted by about 5 km/sec for the average of the strong lines. The weak emission lines are about +1.5 km/sec and the reversals are about 2.5 km/sec to the red of the photospheric lines. The red-shifts are small relative to the line widths. The widths for the weak emission lines are 30 km/sec and that corresponds to an average red-shift of 1.6 km/sec. The stronger lines range from 60 to 85 km/sec in width which corresponds to a greater red-shift of 5 km/sec. The red-shifted Ca II K_2 lines have an even greater width of 170 km/sec. The

line widths correlate with emission intensities and with the strengths of the self-reversals. Another interesting aspect is the time variations that are present in both the calcium and iron lines.

I would greatly appreciate a theoretical explanation of the observed red shift.

Magnan — I would like to describe the profile I call "standard" for an expanding atmosphere. This profile is characterized by a blue-shift of the core and an enhancement of the red emission. The effects are reversed in the case of a contraction. These features are a consequence of a differential Doppler shift along the path of the photon. This shift is due either to a differential expansion in plane-parallel layers or to the curvature of the layers in the case of a constant velocity of expansion.

Underhill — If you take the hydrogen lines in a Be star, invariably the strongest Balmer lines are red shifted with respect to the others, and the velocity of expansion is something of the order of 50-80 km/sec not enough for escape. It is usually stated that H α is coming from a region of smaller outward velocity. The Fe II lines observations might be explained in a similar way.

Wright — This diagram (Figure II-43) is probably the best example we have which shows satellite absorption lines of the K line obtained during the chromospheric phases, prior to first contact, in the spectrum of 31 Cygni. This series was taken at the time of the 1961 eclipse; we hope to obtain another series this summer, chiefly at egress in July. At the beginning of the series, the B spectrum fills most of the K line of the K spectrum and the K₁ and K₂ emission features can be seen. The central chromospheric absorption, in general, becomes gradually stronger as eclipse approaches. A major feature is the appearance of additional satellite lines which come and go. Perhaps the most interesting is the one shown on August 7-28 which showed in nearly the same position for three successive nights when the projected distance of the B star was more than two stellar diameters from the limb of the K star. The feature disappeared but another one appeared again in September and similar effects could be seen right up to eclipse, though after first contact the normal broad absorption of the Ca II K line of the K-type star dominates the spectrum. Similar effects have been noted at eclipses of 32 Cygni and ζ Aurigae; at times I have suspected three or four satellite lines though they are usually weak and sometimes difficult to distinguish from the grains of the photographic plate. The explanation in terms of one or more clouds or prominences in the outer atmosphere of the K star, moving at different velocities, which absorb the light of the small hot B star, still seems to me to be reasonable. These observations seem to confirm to a

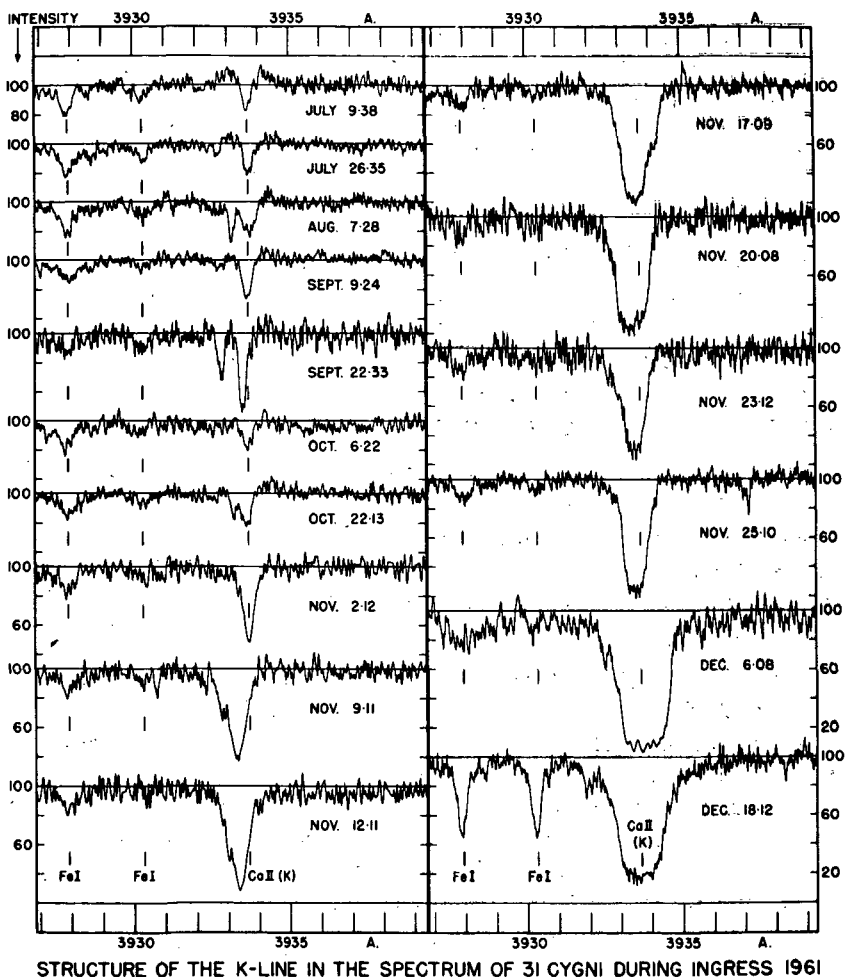


Figure II-43

certain extent the type of phenomena about which Anne Underhill was speaking.

Boesgaard – One point is that these are small shifts compared to the halfwidths. So in fact, there may be more blue-shifted *photons* since the shift is only 5 km/sec while the half-width of the line is 60-80 km/sec and the line-profiles are asymmetric.

Kuhi — One of the problems that has been mentioned is that of mass outflow from the star and its detection by specific line profiles, asymmetric lines, P-Cygni profiles, anomalous line widths, etc. Is there discussion on this aspect of the problem?

Kandel — Are we talking about mass flux as an essential part of a chromosphere or only about mass motions of some sort, i.e., velocity fields, as being a chromospheric phenomenon? These are two different questions.

Kuhi — In Praderie's talk she specifically avoided discussion of mass loss and I think we would like to do that as well since that gets into the problem of extended envelopes and other questions. I should mention Roger Ulrich's defining point this morning, which he didn't get the chance to make, that maybe the outer boundary of a chromosphere is the point at which the material is no longer gravitationally bound to the star. This would eliminate from the discussion all very extended envelopes.

Cassinelli — I would like to point out that it is not necessary to have mechanical energy deposition to have supersonic mass loss. John Castor and I have recently calculated expanding model atmospheres for early type stars. The atmospheres approach the usual static behavior at the base and have supersonic expansion farther out. The only form of energy deposition required for the flow is absorption of radiation.

Pecker — We have been speaking of extended atmospheres and the chromospheres of other women — it seems to me that the point is that an extended atmosphere is defined by its departure from hydrostatic equilibrium so that what is necessary for making an extended atmosphere is to have an additional *momentum* input, while the chromosphere is distinguished from the photosphere by having an additional *energy* input.

Kuhi — Okay, I guess I'll buy that.

Conti — There are many Of stars for which the $\lambda 4686$ of He II line (3-4 transition) is seen in emission and it has always been a mystery why this is so. In at least one star, ζ Pup, the rocket UV observations by Stecher also show the line He II (2-3 transition) at $\lambda 1620$ in emission. And now there have been some observations of the infrared line $\lambda 10124$ of He II (4-5 transition) of that same star by Mihalas and Lockwood, and that line is also in emission. We have however, the He II . . . Pickering series (4-M transitions) in absorption in this star. So some mechanism is overpopulating the ion up to level 5 and then causing cascading down through the other levels. According to the recent models of Auer and Mihalas, they were unable to get the $\lambda 4686$ line into emission and they were certainly unable to get $\lambda 10124$ in emission in any kind of *plane-parallel* model. So it seems very clear that at least for ζ Pup and presumably for all of the Of stars in which you see $\lambda 4686$ in emission, you must have some sort of extended envelope. If there was a planet from which some ζ Puppian were watching their Sun, and there was a solar eclipse by an

appropriate moon, they would certainly see *chromospheric* emission lines in He II, but that's an aside. The main point I want to make is that when you see $\lambda 4686$ in emission, there is some sort of extended envelope around the star.

The star I want to talk about now is θ_1 Ori C. As some of you may know, this is the central star of the Trapezium and the star that excites the Orion nebula. I have some spectra to show of this star. Figure II-44 shows

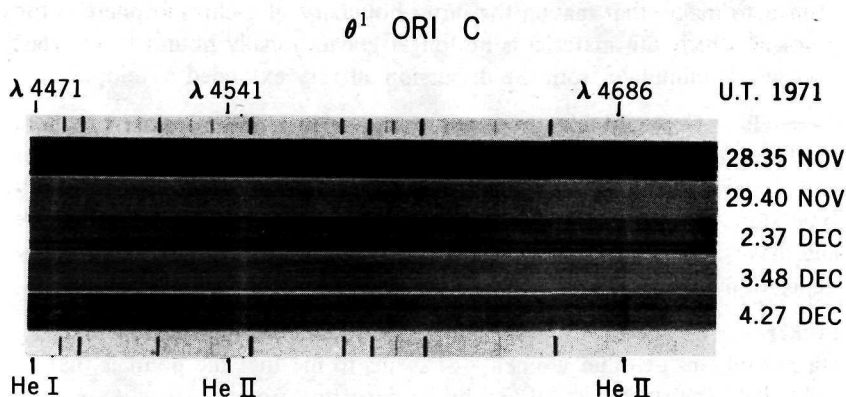


Figure II-44

the spectral region of $\lambda 4471$ and 4541 of He II. These show just as absorption lines on these spectra, taken on five nights during one week. Note the appearance of $\lambda 4686$, the first two nights. Then a couple of nights later we see an emission at $\lambda 4686$. The emission is violet displaced and the absorption is red displaced, and we call this an inverse P Cygni profile. As most of you know, a P Cygni profile is one which suggests that material is flowing out from the star. Therefore an inverse P Cygni profile suggests the opposite. Figure II-45 shows the profile of $\lambda 4686$ on the first two nights. The absorption line is undisplaced with respect to the other absorption lines and then after three nights we see the emission on the violet and the absorption on the red side. What this suggests on the face of it is that there is material which is falling into θ_1 Ori C. Sometimes material is accreting and other times it isn't. That is an interesting phenomenon for a star that has excited a gigantic nebula which is apparent to the naked eye. There are a number of physical problems connected with that process, and I think the line formation problem is the presence of accretion is in itself an interesting problem for astrophysics. The terminal velocity for material falling in is about 1100 km/sec and the infall velocity, roughly given by the absorption profile, is

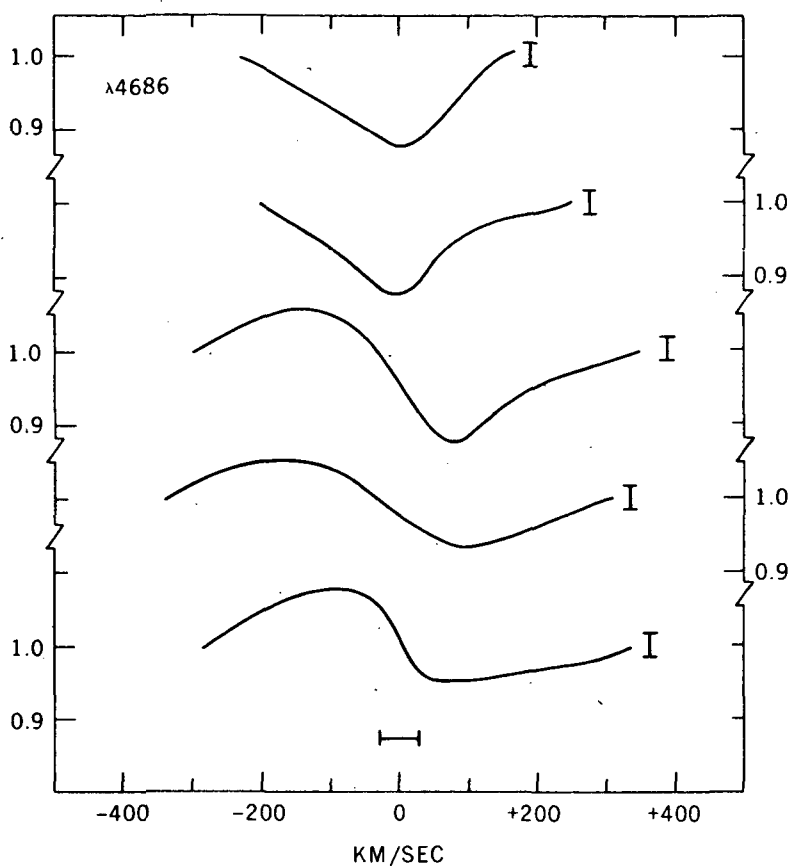


Figure II-45

something like 10 or 20% of that. So it isn't coming in with full force; presumably radiation is braking the fall, but it is definitely accretion. That should lead to some interesting problems of interpretation.

Wilson — Some of those θ_1 Orionis stars are binaries, are they not?

Conti — This star is listed as a spectroscopic binary. Upon searching the literature, you find out it is called a spectroscopic binary by Frost, et al. They give it this identification on the basis of "large" velocity variations, back in the 20's. Then you find that the senior spectroscopist, Struve, (and Titus in 1944 also studied this system) could find no velocity variation that could be blamed on binary motion. The plates I have, which are these five and another eight or so, all show no velocity variations.

Wilson — That might be why the famous Wolf-Rayet star that was an eclipsing binary stopped.

Conti — Once a binary always a binary.

Kuhi — Yes, but it stopped eclipsing.

Conti — But it didn't stop being a binary.

Pasachoff — Let me show you some observations we've been making with the 100-inch telescope on Mt. Wilson, using the 32-inch camera of the Coude spectrograph at 6.67 Å/mm. Wilson and Ali (P.A.S.P. 68, 149, 1956) observed the helium D_3 line a few years ago and reported a probable detection of D_3 in four stars, namely ϵ Eridani, 61 Cygni A, κ Ceti and λ Andromedae. The first three are dwarfs and λ Andromedae is a spectroscopic binary with a strong chromosphere. However, they were able to measure the position of the supposed D_3 line for three of the stars, and found that they were displaced some 0.4 Å or so to the red. Since this region is confused by the presence of some water vapor lines all around D_3 (at $\lambda 5875.44$, 5875.60 , and 5876.12 , for example, with D_3 at $\lambda 5875.64$ right in the middle) the evidence was still incomplete.

Since that time, Vaughan and Zirin (Ap. J. 152, 123, 1968) have published a paper with image tube observations of $\lambda 10830$. Thus we now know for a variety of stars what the velocities may be. In fact, a dominant red shift effect does not appear. Some stars do show such velocities, but they are not always in the same sense. Figure II-46 shows the triplet energy diagram. The $\lambda 10830$ line comes from the metastable 2s triplet state and the $\lambda 5876$ triplet goes from the 2p state, 1.14 eV higher, up to the 3d state.

We have observed a variety of stars, a few A and B stars but mostly G, K and M stars. Just as Wilson could not specify results for the M stars because there were too many lines in this region to know whether what is seen is D_3 or not, we also had to limit ourselves to the G and K stars. But we do not know which stars have strong $\lambda 10830$. β Draconis, for example, a G2 II star, has 1000 milliangstroms of $\lambda 10830$ according to Vaughan and Zirin. Zirin has some more recent, unpublished observations showing twice that equivalent width. Looking at the D_3 wavelength on Figure II-47, you see that there is no line there. For β Scuti, similarly, there is no D_3 radiation. For λ Andromedae it is a little trickier. There is an iron line about an Angstrom to the red, which could broaden the total profile, but there is probably a line near the basic D_3 frequency. λ Andromedae has 1000 milliangstroms or so of $\lambda 10830$ and it is, of course, a spectroscopic binary. There is even a hint on one plate of possible emission around D_3 , though that certainly remains to be confirmed.

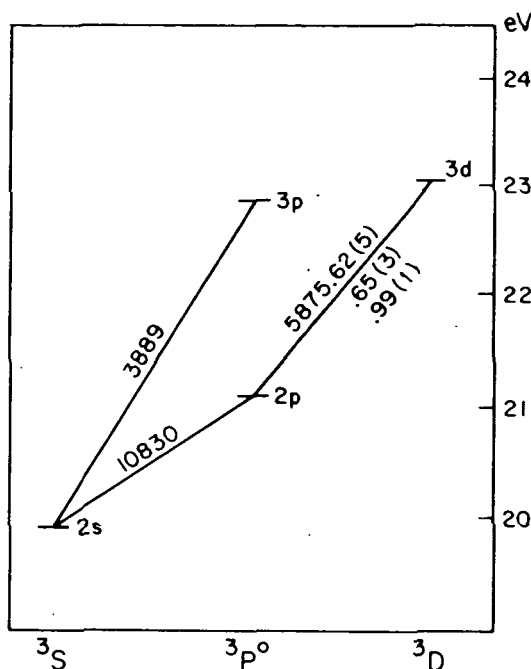


Figure II-46

λ Cygni, a K5 lb star, does not show clear D_3 in that region. We have to follow these stars at different times of the year to use the different radial velocities to separate out the atmospheric contamination. We are continuing this project.

Figure II-48 shows some of the spectra. First of all, for β Orionis at the top, there is a strong D_3 line. It is not "chromospheric," according to my definition of a chromosphere, for this is a B star and I would tend to call it a hot atmosphere. The other stars do not show this line, except for λ Andromedae, which does show a possible faint line and even possible emission on this plate. However, β Draconis, a G2 II star, may have twice the $\lambda 10830$ of λ Andromedae yet it does not show D_3 absorption, certainly not of the magnitude of λ Andromedae.

So what I would really like are comments on theoretical calculations of the relative intensities one expects for $\lambda 10830$ and D_3 . You might expect D_3 lines to be down by a factor of perhaps 10, calculating with a dilution factor of 2 for an atmosphere of about 6000°K . The ratio will change as we go to cooler atmospheres, but it would be nice to have some more exact model calculations from all the people calculating grids. We would also be happy to have suggestions for additional stars to observe in our continuing observing program. Elliot Lepler, a graduate student at Caltech, has cooperated in this work.

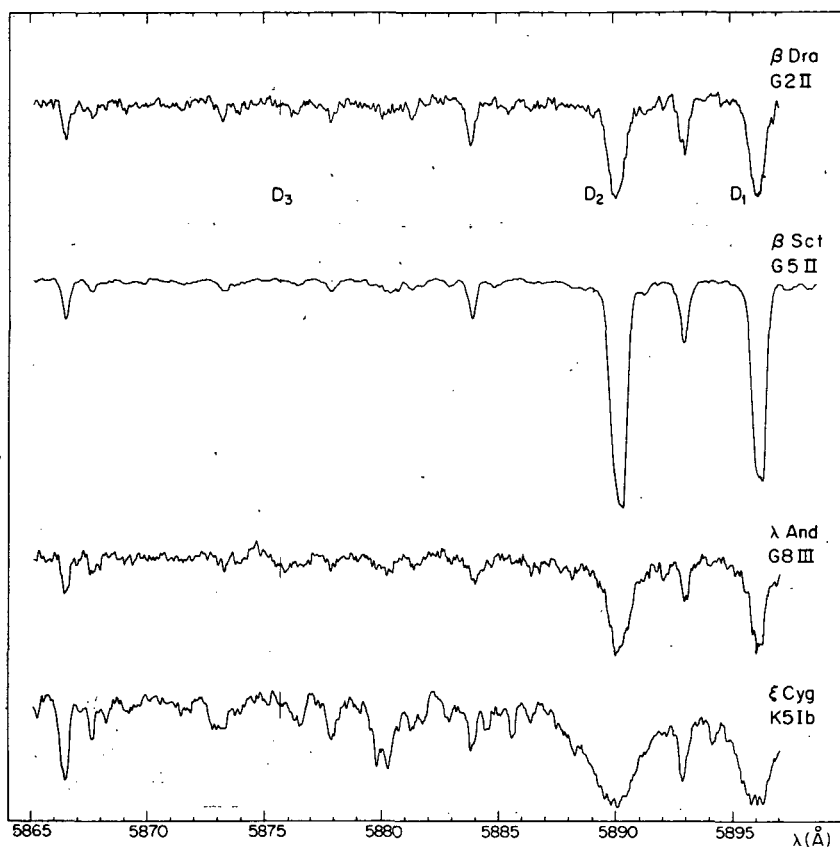


Figure II-47

Fosbury — In Vaughan and Zirins' original paper they suggested that you are more likely to see 5876 in the slightly earlier type stars, i.e., the F stars rather than G. This is the case of Zeta Doradus again.

Pasachoff — Vaughan and Zirin did comment that they found $\lambda 10830$ in one F star which surprised them. I have not observed F stars yet for D_3 .

Underhill — If you are looking for chromospheres in the F, G, and K stars, certainly in the low chromosphere, where there are reasonable densities, the most prominent ions are singly ionized metals and some of the neutrals. We already found out that non-LTE applies here because the density is a bit too low for LTE to apply. Most of the resonance lines that we would want to look at are located in the ground-based region of the spectrum. Doherty showed us that it is very difficult to get an observable flux in the UV but there are really not too many low-

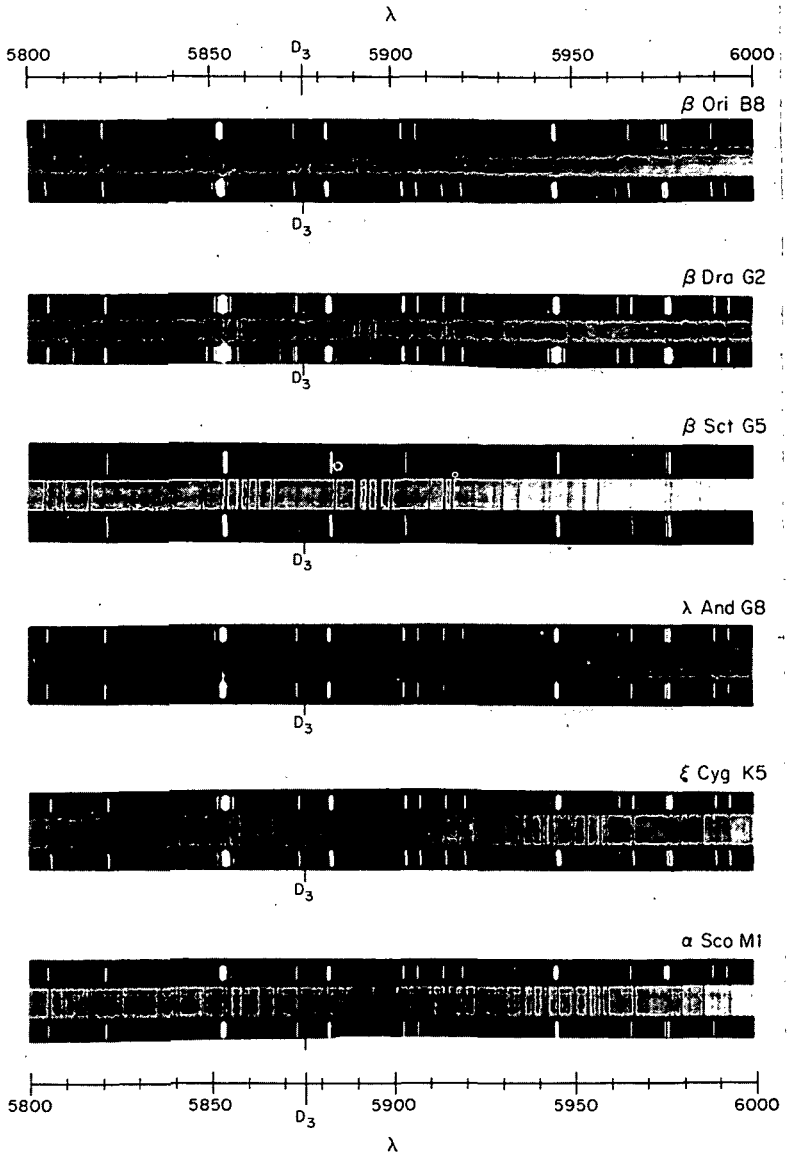


Figure II-48

chromosphere resonance lines there, so we are not too badly off down to about 2000 Å which is an easier region to observe than the region below 2000 Å. I think the region 2000 Å to 3000 Å is important because there are a lot of Fe II, Cr II, etc., lines. If you go to the A stars you get Si II,

C II, between 1000 and 2000 Å. So the near-ultraviolet is not a difficult region for good observations of stellar chromospheres, and it is tragic that we have not got before us any observational capability for the near future to observe such regions. We are going to have to rely on balloons and rockets which have limited capabilities.

Kondo — I would like to point out that high altitude balloons can be useful for observations down to about 2000 Å and do offer long observing periods in comparison with rockets. Residual extinction can still be a problem, however, for observations of certain types, particularly near 2500 Å where the absorption due to ozone is high.

Doherty — It might help if I pointed out that the slide I showed with the decrease of many magnitudes was for broad band measurements. In the case of the later stars, these results do not show any of the emission lines that might be stronger. The fact that we have a measurement at all of Ly α Arcturus is somewhat remarkable and is evidence that Ly α is a very strong line in that region.

Gros — Through an analysis of the observed radiation of Sirius (A LV) in two wavelengths located as far as possible in the ultraviolet spectrum, we have tried to derive information on the thermal structure of the superficial layers of the atmosphere of this star. We have used measurements made by Carruthers (1968) at $\lambda_1 = 1115$ Å and $\lambda_2 = 1217$ Å.

- *Analysis* — Applying the Eddington-Barbier approximation and assuming that the source function at the observed wavelengths follows the Planck function, we have deduced the temperature gradient between the layers ($\tau_\lambda = 2/3$), where the radiation at λ_1 and λ_2 is formed from the knowledge of the ratio of the observed fluxes $F(\lambda_1)/F(\lambda_2)$. To get the depths of formation at 5000 Å for the radiation at the relevant wavelengths, we need a model to start with: we have adopted an LTE, non-gray for the continuum, radiative equilibrium one.
- *Application* — The first approximation model is homologous to a model due to Strom, Ginerich, and Strom (1966), with an effective temperature $T_{\text{eff}} = 10486^\circ\text{K}$ and a surface gravity of 10^4 . The chemical composition was deduced from the study of lines in the Sirius visible spectrum: silicon is overabundant by a factor of 17 relative to Warner's (1968) solar abundance. The observations (Carruthers, 1968, Stecher, 1970, OAO scans) and the theoretical spectrum from the Strom et al. model are plotted in the Figure II-49. We must point out that, in the absence of an absolute calibration for the OAO data, we have related them to the ground based observations of Schild, Peterson and Oke (1971). The

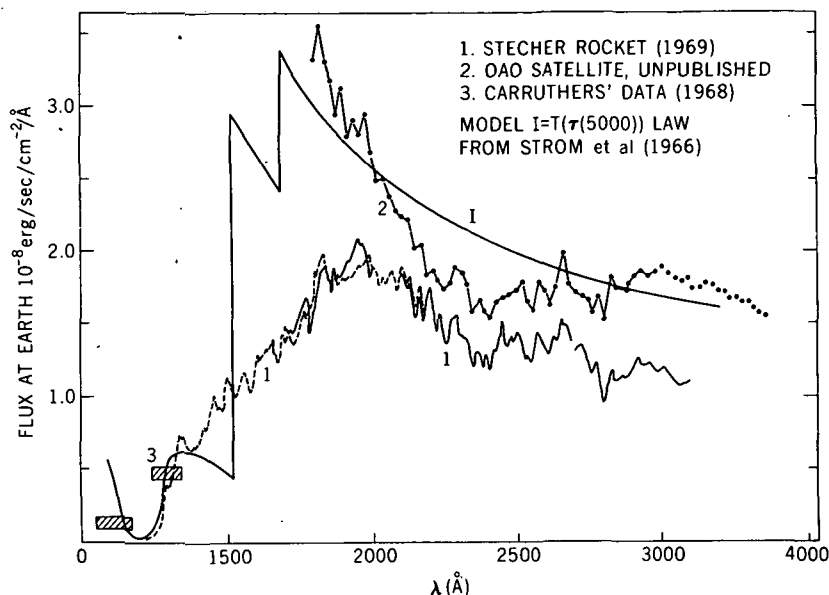


Figure II-49

comparison between the observations and the predicted fluxes shows that the spectral region around Ly α is well fitted by this model, the computed flux is too low between 1300 Å and 1520 Å, and at 1520 Å, a discontinuity due to photoionization from the 3P level of Silicon is present ($\Delta m = 2.07$ mag). This discontinuity is not shown by the observations. This discrepancy had been pointed out by Gingerich and Latham (1969).

The model allows us to compute the depths of formation for the continuum at each λ as shown in Figure II-50. Note that the violet side of the Balmer discontinuity is formed at about the same depth as the UV radiation at wavelengths greater than 1430 Å. This is the main difficulty of the application of the present method to stars as hot as Sirius.

One gets the following results :

$$\lambda_1 \quad \lambda = 1115 \text{ Å} \quad \tau(5000) = 0.086$$

$$\lambda_2 \quad = 1270 \text{ Å} \quad \tau(5000) = 0.066$$

$$\Delta T_e = +190^\circ\text{K}$$

Hence we derive an increase of the electronic temperature T_e in the outer layers starting at $\tau(5000) = 0.086$.

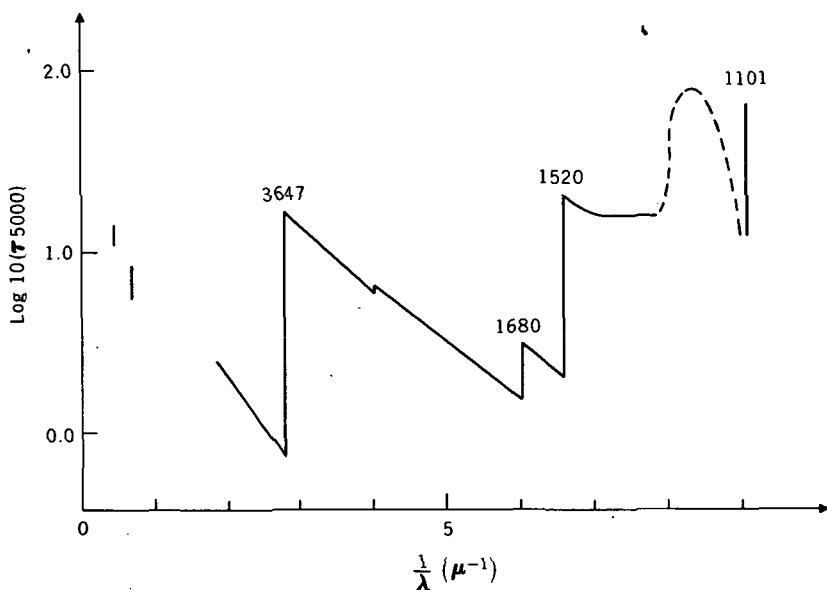


Figure II-50

Figure II-51 shows the semi-empirical model obtained from the Strom et al — one by modification of T_e ($\tau(5000)$ above $\tau(5000) = 0.086$). The characteristics of the predicted flux from this model are shown on Figure II-52.

- A good fit exists for the region around Ly α .
- Between 1300 Å and 1520 Å, there is a small excess of flux, which is compatible with the presence of strong lines shown by Stecher's observations.
- The computed Si I discontinuity at 1520 Å is still too large, but it has been decreased by a factor of 2 ($\Delta m = 1.26$); the same is true for the Si I discontinuity at 1680 Å. We have shown elsewhere that the discontinuity at 1520 Å is blurred by the strong Si II doublet, at 1526 Å — 1533 Å.
- This model is too hot to fit the observations (OAO spectrum) between 2510 Å and 3647 Å. Moreover the computed Balmer discontinuity is too small ($D = 0.27$), as can be seen on Figure II-53.

We conclude that this attempt is not completely satisfactory in two respects:

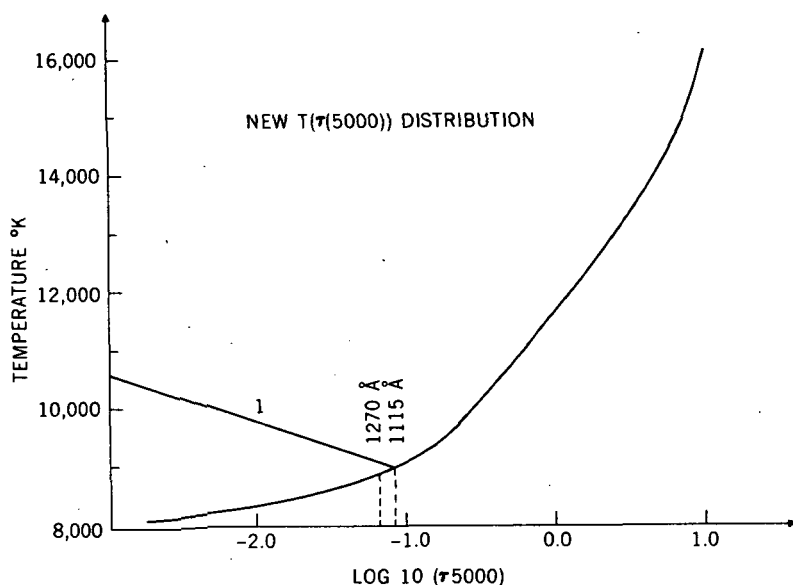


Figure II-51

- The analysis has been carried out with a purely LTE source function for the continuum at 1115 Å and 1270 Å; at those wavelengths the opacity is actually due simultaneously to the wing of the Lyman α line, and to the CI and Si I continua; the total source function implies then the knowledge of the departure coefficients in hydrogen, carbon, and silicon atoms.
- The depths of formation for the radiations we use in our analysis depends on the model we choose to start with. If this model has a lower temperature T_0 at the surface we can hope that the concerned layers will be higher in the atmosphere and that we will so avoid affecting the formation of the blue side of the Balmer discontinuity. A complete multiple iteration must be performed.

We thank Dr. A. D. Code and Dr. R. C. Blen for having provided us a spectrum of Sirius in the region 2000 – 3500 Å, from the OAO satellite.

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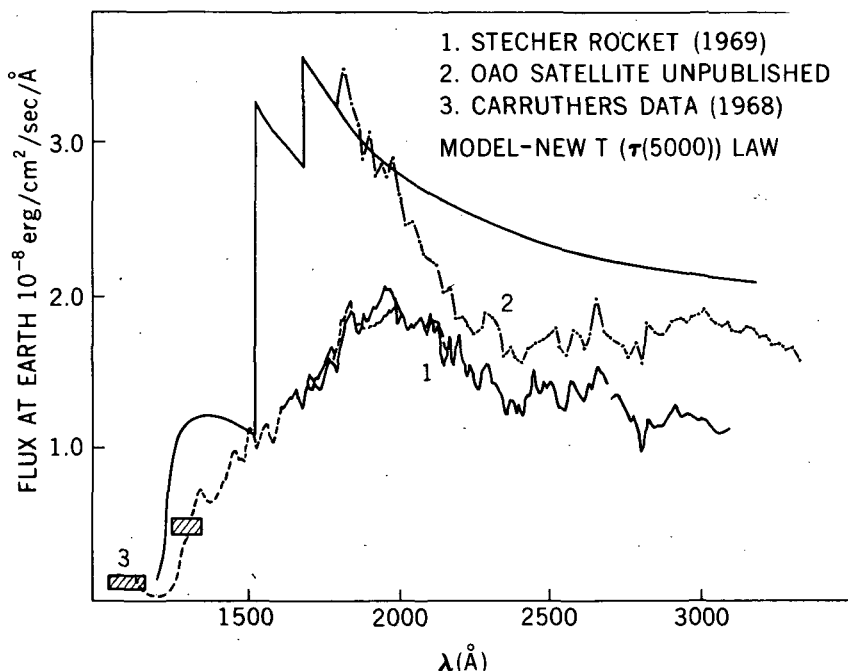


Figure II-52

Stecher, T. P., 1970, *Ap. J.*, **159**, 543.

Strom, S. E., Gingerich, O., Strom, K. M., 1966, *Ap. J.*, **146**, 880.

CONTINUATION OF DISCUSSIONS FOLLOWING TALK BY PRADERIE AND DOHERTY

Underhill — I have some of the observations of Sirius from OAO and from ground-based work. The OAO results tend to be overexposed so I have not used them. There are Stecher's rocket observations, and Dennis Evans from the Goddard Optical Astronomy Division has done an absolute calibration of the rocket scans with effectively the same instrument but with independent absolute calibrations. This material was presented last summer but has not yet been published. Those two sets of measurements agree within the uncertainties of transfer to absolute intensity, within 15%, say. The OAO results for this star can't be calibrated as well as rocket data. I have not heard from Savage in Wisconsin what he thinks of my revision of his sensitivity curve based on the rocket data.

Sacotte — In the preceeding talk, M. Gros had some observations in the Lyman α range and she obtained some models. In this work, we are

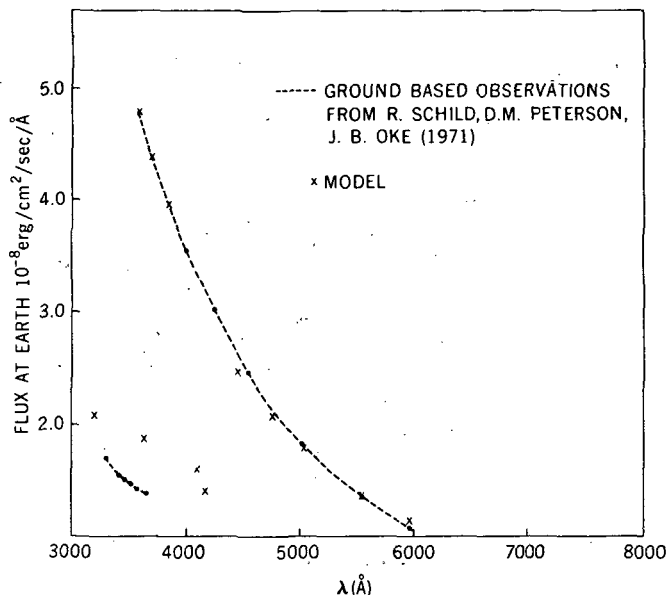


Figure II-53

departing from models. We compute some synthetic spectra and compare them with observations in the range 2000 – 3000 \AA , and then use characteristics of the models for comparison. We used the OAO results and believe the calibration is accurate to about 20%. To compute a synthetic spectrum we compute the emergent flux every 1 \AA or 0.5 \AA and then convolute the emergent flux by the apparatus function, and we obtain a spectrum directly comparable to the OAO spectrum. Line calculations are made in LTE. We assume that the source function is the Planck function and we use atomic data from various sources. We use a broadening constant 2 times the classical value plus the effect of broadening by hydrogen and helium. The first graph, Figure II-54, shows the OAO spectrum and the comparable synthetic spectrum. The model used is by Strom, et al. From 2000 \AA to 2500 \AA we have an important disagreement, but in both spectra we notice important absorption features. At λ 2500 \AA , we introduce in our line computation the data on Fe II by Warner and the agreement is much better as a result. The level of observed flux is reached and every feature is well reproduced. The second graph, Figure II-55, shows a similar computation based on the model of M. Gros, and here the agreement still is not good. We can reproduce various changes in the spectrum but the flux levels are not in agreement, and we can see some emission levels in our calculation. All we can deduce

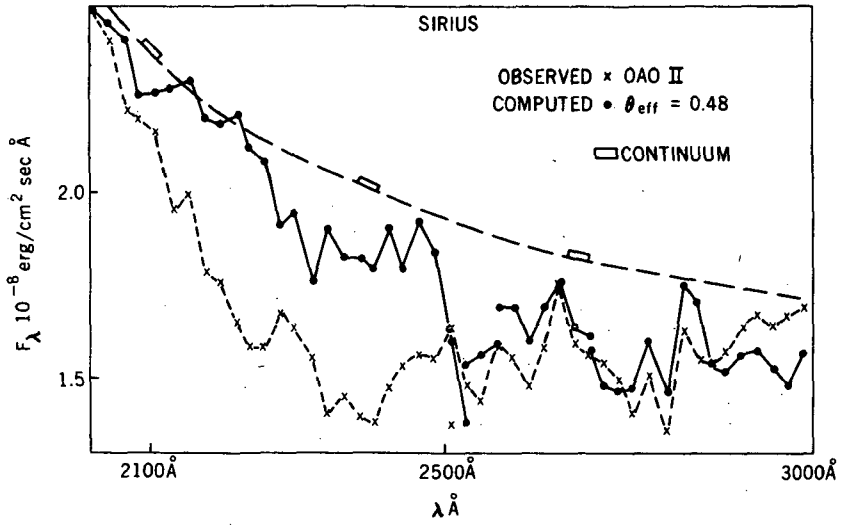


Figure II-54

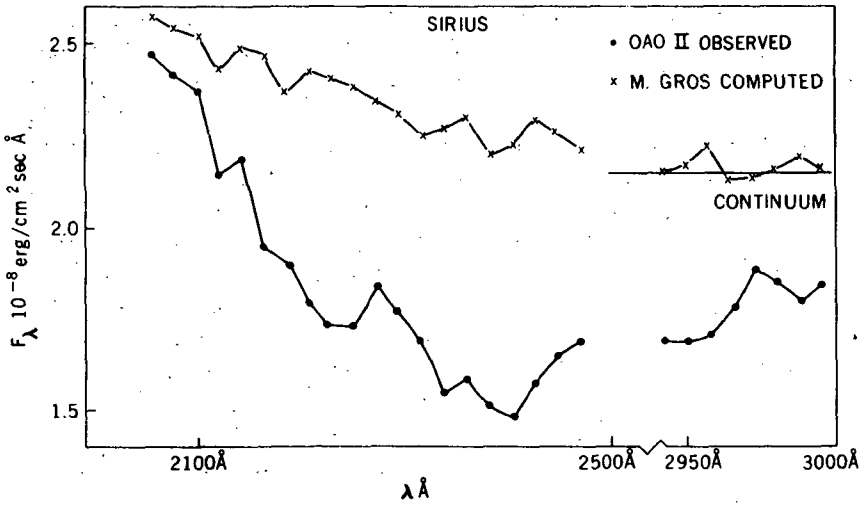


Figure II-55

from the calculation performed with the semiempirical model of M. Gros is that the location and the importance of the rise in temperature deduced from the Ly α region in a A1 Star have very sensitive effects redward of 2500 Å to the Balmer discontinuity.

Giuli — I would like to amplify a comment made earlier by Kondo on the use of balloons in UV astronomy to about 2000 Å. Current operational balloons can carry telescopes with easily twice the light gathering power that any Aerobee type rocket can carry. At altitudes of something like 40 km the signal strength or recorded signal per unit time is just as strong as that of a rocket and on top of that you have the advantage of an entire night's observing time. For some reason astronomers seem to have missed out on many of the recent developments. Cosmic ray physicists have been using balloons for a long time, but there are really only about three astronomy payloads that have seriously considered ballooning for ultraviolet astronomy. I would like to encourage those of you who are seriously interested in ultraviolet astronomy from balloons that there are several places around the country which can offer advice, based on experience, such as the Gehrels Polariscope group in Tucson or our group at the Manned Spacecraft Center.

Bonnet — We have also been using balloons to perform ultraviolet studies of the Sun.

Peytremann — Such balloon experiments have been carried out for many years by smaller countries with limited research budgets, and these were often considered secondary relatively unimportant experiments compared to the orbital ones. There is perhaps some irony in the renewed interest shown here in ballooning.

Giuli — Yes, my comments have been directed to the American astronomers. It is ironic, but understandable, how balloon astronomy has been neglected in this country. Our space program was funded suddenly, and, as a consequence, funds were suddenly available for rocket payloads.

Aller — Can balloons take payloads above the ozone?

Giuli — Reliable balloons can carry 500 kg payloads to 40-42 km. Smaller telescopes could be carried reliably to 44 km, but to go much higher requires a tremendous increase in balloon volume, and hence cost and risk. Also, the larger balloons obscure a larger portion of the sky about the zenith.

Kondo — It is also important to realize that the state of the art in carrying out these experiments has advanced significantly in recent years. Sophisticated pointing and stabilization systems used in our experiment are examples of such an advance. One can also benefit greatly from the

capability to monitor in real time the spectrum being scanned, using only the integration time needed for this purpose, and then moving on to observe other objects. The flexibility we now have in carrying out observations was not available a few years ago.

Kuhi — That's an important point; ballooning is not restricted to photographic recording of data.

Underhill — I agree with these possibilities for good observations down to 2000 Å or so. Most of the emphasis in ultraviolet astronomy has been on the hot stars and on the wavelength range below 1700 Å. I have felt like a voice crying in the wilderness saying that more stars can be observed and more interesting things in the 2000-3000 Å range than have received attention so far. But let us not, please, lose sight of the fact that you really need a spectroscopic satellite, such as we have described as SAS-D, up there to observe all sorts of stars for a long time. Balloons and rockets have their place, but satellites are required for comprehensive observing programs.

Bonnet — I would like to add a comment on balloon spectroscopy. We took advantage of balloons to look at the solar spectrum but had no means at that time to look at stars, due to the lack of a good pointing system. It appears possible to observe at balloon altitudes in the range below 3000 Å down to 2700 Å. Below that wavelength you have a strong absorption by ozone and at lower wavelengths competition between absorption by molecular oxygen and ozone. However, there is a reasonably transparent region between 1900 Å and 2300 Å and, furthermore, this region of the solar spectrum is very interesting because of the presence of the carbon emission line at 1994 Å. Detailed observations of this line shows that it is emitted in very limited regions, probably corresponding to spicules on the Sun. If this is confirmed it would be possible to look for spicules in stellar spectra by observing the carbon line using balloon spectroscopy. This line is quite strong and might help in identifying a rise in temperature in the outer layers of a star as well.

Jefferies — In Hawaii we have been making ultraviolet spectra of the Sun from a rocket, and with a resolving power of about 200,000. One of the lines that we have observed is a line of S I; this displays a very curious distribution over the Sun. It is extremely strongly limb brightened in our spectra. I forget the exact wavelength, but I was wondering if any of the stellar observers have seen this. It is a reliable observation and seems a definite indicator of some sort of chromospheric emission.

Kuhi — Apparently no stellar observers have seen this line.

Fosbury — Can I comment on the point that Jefferies just made. Let us refer to the diagram for the Ca H and K lines — and the Mg II emission lines. I am trying to find out whether it is a Doppler effect or an opacity effect. I know there is an Fe line blending with the H lines, but can you not do that with the H and K lines separately? You have a factor of 2 in oscillator strength.

Jefferies — In principle you should be able to do so, but, in practice, I don't think it will work. I think that we need a much larger factor than 2 between the optical depths to show a difference of the kind you mention. I think that the factor should be about 10 between Mg and Ca.

Pasachoff — May I make a plea for not confusing the spectroscopic notation for H and K, which refer to Calcium. I suggest that we find other names for the Mg resonance lines.

Kondo — We are provisionally calling those lines the 2795 line and the 2802 line. However, we might also consider alternative ideas such as use of "h" and "k" suggested by Skumanich.

Kuhi — Fraunhofer's notation ends up by P, so we could use P and Q. They are resonance lines and the least confusion is caused if we refer to them by wavelength rather than by the Fraunhofer notation which has caused enough confusion.

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PART III

MECHANICAL HEATING AND ITS EFFECT ON THE CHROMOSPHERIC ENERGY BALANCE

Chairman: Andrew Skumanich

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MECHANICAL HEATING IN STELLAR CHROMOSPHERES USING THE SUN AS A TEST CASE

Stuart D. Jordan
*Laboratory for Solar Physics
Goddard Space Flight Center*

INTRODUCTION

The remarks in this talk will apply only to chromospheres of comparatively late type stars which have significant convective envelopes. This is not to imply that mechanical heating does not occur in other stars, but only that, to the best of my knowledge, little or no satisfactory progress in applying mechanical heating theories to the outer atmospheres of non-solar type stars (without convective envelopes) has been made. Indeed, practically all of the progress that has occurred has been in solar work, so most of my remarks will pertain to the Sun.

The serious work on solar atmospheric heating began in the late 1940's and, since then, has included treatments of wave modes which might be involved and the development of observational techniques to detect them. Definite results up to the mid-1960's included strong theoretical support for some kind of gravity-modified sound wave as the source of at least some heating via shock dissipation, and the earliest observations of the now well known (but still not well understood) 300 sec periodic variations in the line central brightness and position of many upper photospheric and low chromospheric lines.

Comparatively recent efforts in the past six years have emphasized more detailed numerical calculations, including some non-linear effects, to determine the generation, propagation, and dissipation of various wave modes for more realistic solar atmospheric models. In addition, the corresponding observational work has been directed toward studying phase relations among oscillations at different heights (using lines of different strengths) and toward getting both high spacial (1 arc sec) and time (5-10 sec) resolution spectra, in the hope of inferring directly from the observations information on the heating and the associated velocity fields.

With that background, I'd like to offer a brief review of the principal wave modes proposed and studied for the heating, along with where they are generated and how they propagate. Then I'll review the solar heating picture as it stands today.

WAVE GENERATION AND PROPAGATION

The general problem of wave propagation in a compressional atmosphere with gravity and a magnetic field is treated by Ferraro and Plumpton (1958) and many others. Since it is difficult to solve the propagation equation with all the terms in it, the usual procedure has been to obtain solutions for simpler cases where one or more of the three basic parameters (medium compressibility, magnetic field, and gravity) are left out. For the moment, I'll ignore the magnetic field parameter.

Extensive studies of the gravity-modified sound wave have resulted from the original suggestions of Biermann (1946) and Schwayschild (1948) that these waves heat the outer atmosphere by shock dissipation. In particular, numerous applications of the Lighthill (1952) theory for generation of sound waves by isotropic turbulence have followed his pioneering work. One comparatively recent and important contribution by Stein (1968) included several calculations of both the total acoustic power generated and the frequency distribution of the acoustic emission. To do this calculation, it is necessary to know the turbulent velocity amplitudes and also the turbulence spectrum (spacial and frequency dependence) in the generating region. Since these parameters are currently difficult to infer from observations, reliance on a convection zone model and theoretical turbulence spectra is necessary. Stein, like many others before him, had to use an admittedly rough model for the convection zone, based on the earlier Böhn-Vitense (1953) mixing length theory. He then did the calculation for several different turbulence spectra. His results demonstrated that the total acoustic power output is highly sensitive to the high frequency tails of these spectra. This situation, added to the already well known sensitivity of the result to the turbulent velocity amplitudes (the acoustic emission varies as the fifth power of the turbulent Mach number), introduces considerable uncertainty into the computed acoustic flux. Stein's computations yielded an uncertainty of about an order of magnitude in the acoustic flux, but the further uncertainties in the convection zone model and in the method used for the calculation, which ignored the interaction between sound and turbulence, suggests an even greater final uncertainty in the results.

In spite of all these difficulties in this extremely elaborate treatment, Stein's results are important for two reasons. First, even if his *lower* limit for the upward flux of sound waves is an *overestimate* by an order of magnitude, this flux would still be of the order of 10^6 ergs cm^{-2} sec^{-1} , which now seems adequate to balance the net radiative losses in the lower chromospheric region by dissipation of weak shocks. Since the empirical evidence of the solar granulation, as well as simple theoretical arguments based on Rayleigh and Reynolds numbers, lends continuing support to

this general picture of sound wave generation at the top of the convection zone, Stein's results are encouraging. Second, the calculated frequency dependence of his acoustic emission exhibits a peak far above the critical angular frequency $\omega_s = \gamma g / 2c_s$ (γ = specific heats ratio, g = gravity acceleration, c_s = sound speed) below which all sound waves are reflected. If this were not true, vertical transport of the sound waves through the temperature minimum could not occur. This important result was true for all turbulence spectra used. Figure III-1 is a graphic demonstration of this second conclusion, where acoustic flux spectra are graphed for the three turbulence spectra used by Stein. An immediate consequence of this

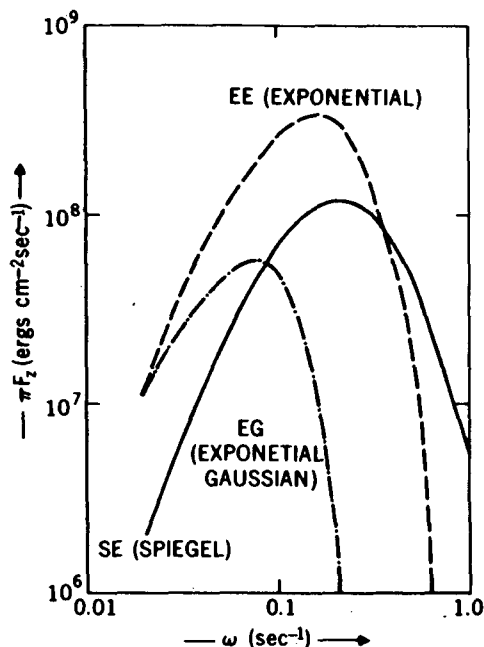


Figure III-1 Steins solar acoustic flux spectrum.

result was that people working on the chromospheric dissipation of waves generated by turbulence in the low photosphere returned to their work with renewed confidence that they were doing something relevant to the Sun. The general picture of chromospheric heating now seems still more involved than when Stein's results appeared, as we shall see presently, but the two main conclusions mentioned still stand, to the best of my knowledge.

So far, I have deliberately avoided mentioning magnetic fields. We know they must play some role in the heating problem. One has only to note the strikingly different behavior in the temperature sensitive H and K

lines over plages and the so called normal chromosphere. What role do the magnetic fields play?

This is a difficult question to answer, because the introduction of the magnetic field complicates the mathematical problem considerably, particularly by introducing significant non-linear terms into the propagation equation (Pikel'ner and Livshitz, 1965). Understandably, less progress has been made here than in treating the simpler case of zero magnetic field. Fortunately, there is one rather strong statement that can be made. It *may* be possible to ignore the magnetic field and still obtain a relevant model for the solar chromosphere. By relevant, I mean an approximate, one-dimensional, theoretical model, based on a mechanical heating theory which ignores magnetic fields, and yet, which is in substantial agreement with one-dimensional models derived from observational data. If this proves true, it would have direct bearing on the theoretical treatment of chromospheres of non-solar, main sequence stars with convective envelopes. Difficult as it may be to devise ways of computing non-radiative equilibrium models for these stars with a relatively simple heating theory it would be extremely difficult to do it with the non-linear (and, possibly, multi-dimensional) aspects the problem would assume with strong magnetic fields.

To demonstrate this simplifying possibility, consider the dimensionless parameter

$$\frac{c_A}{c_s} = K \frac{B}{\sqrt{\rho T}}$$

where c_A and c_s are the Alfven and sound speeds, respectively, and B , ρ , and T are the magnetic field strength, mass density, and kinetic temperature. The quantity K is an almost constant function of the mean molecular weight and the specific heats ratio. From the wave equation for propagation in a medium with magnetic field, we can readily see that, when $c_A/c_s < 1$, the wave propagates more like an ordinary sound wave as the ratio becomes progressively smaller. In the language of Osterbrock's (1961) well known study, the fast hydromagnetic mode becomes the sound mode. But it is easy to substitute the appropriate numbers to see that this is exactly what happens in the solar low chromosphere and photosphere outside of plage and spicule regions, which comprise a small fraction of the total gas mass at these heights in the atmosphere. So, barring the possibility that the magnetic structure of the bulk of the gas is a small scale, unobservable, high-fields-of-opposing-polarity situation, it

follows that, below and possibly within much of the transition region, the heating occurs mainly in regions of negligible magnetic field.

These remarks are meant only to show one way in which the magnetic field might be negligible in treating one part of the heating problem. As chromospheric densities drop rapidly with height, we soon enter a situation, somewhere in the transition region, where $c_A/c_s \gg 1$, even for a field of one gauss. Also, any treatment of heating in plages and spicules requires inclusion of magnetic field effects. Finally, the magnetic field will play some role, perhaps a vital one, in wave generation (cf. Kulsrud, 1955), again, where $c_A/c_s \gg 1$. So the current research on how to treat various hydromagnetic modes and their interactions with each other and the non-uniform propagation medium is very important and should certainly be pursued vigorously. On the other hand, the comparative insensitivity of the solar wind to the solar cycle (Hundhausen, 1968) suggests, though it does not prove, that at least the total amount of steady state mass and mechanical energy flux from the subphotospheric regions is constant and, thus, not strongly dependent on magnetic activity. Perhaps many (important) details of the steady state heating will prove to be strongly dependent on the magnetic field, while the total magnitude of the heating will not. These are major questions for which we currently lack answers.

Another wave mode that has been treated extensively as a possible heating mode is the gravity wave, the relatively low frequency, long wavelength, two dimensional wave characterized by elliptical (rather than longitudinal, as in the case of sound waves) particle motion in the vertical plane passing through the wave propagation vector. This mode represents one possible solution of the wave equation, leaving out the magnetic field, but including medium compressibility and gravity. Given a suitable perturbation, this mode is certainly present in the solar atmosphere wherever the radiative relaxation time is not too fast to suppress it. Whitaker (1963) injected the gravity wave into the solar heating problem because sound waves with (relatively low) frequencies characteristic of photospheric granules (Bahng and Schwarzschild, 1961) could not propagate through the temperature minimum region. This was before Stein showed that the frequencies for sound waves generated by the Lighthill mechanism lay much higher than the critical cut-off frequency $\gamma g/2c_s$. Thus, Whitaker's original motivation for proposing the gravity wave no longer exists.

This situation can be illustrated by the diagnostic diagram in Figure III-2. This diagnostic diagram is simply a plot of the dispersion relation $F(\omega, k_x) = 0$ for different vertical wave numbers k_z and a set of physical parameters characterizing the solar temperature minimum region. (Mean

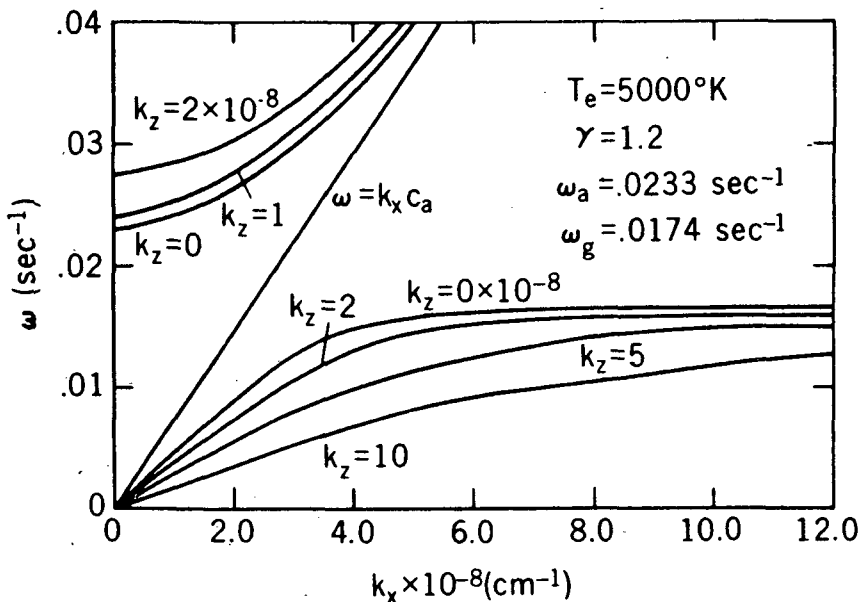


Figure III-2 Diagnostic Diagram for T_e (min) region.

molecular weight unity is also used.) The values given are those chosen by Whitaker, but, although T_e should be lower, it doesn't change the general picture. ω_g is the Väisälä-Brunt frequency above which vertical propagation of gravity waves cannot occur. It is given by $\omega_g = g(\gamma - 1)^{1/2}/c_s$. The straight line solution $\omega = k_x c_s$ is a pure sound wave in a zero gravity medium, that is, a horizontal sound wave in the Sun. The solutions in the upper left-hand corner represent the gravity modified sound waves which, as we see, cannot propagate vertically for $\omega < \omega_g = .0233 \text{ sec}^{-1}$. Thus, for example, a 300 sec sound wave could not propagate up through this region. Of course, now we believe that 30 sec is a more representative period for the high frequency sound wave, and this latter period lies well below the limiting value for vertical propagation. The gravity waves, on the other hand, have dispersion relations more like those of the photospheric granulation with which Whitaker seems to have identified them. Hence, we see his preference for gravity waves. In addition to the fact that the gravity waves no longer seem necessary in the low photosphere, there is a more serious objection to associating them with this region. That is, as Souffrin (1966) pointed out, the rapid radiative relaxation time, of the order of one second, would quickly eliminate these oscillations in this region.

It would seem that gravity waves play no role in solar atmospheric heating and that the preceding discussion is somewhat irrelevant, but this is not necessarily the case. There is now convincing observational (Frazier, 1968) and theoretical (Moore, 1966) evidence that a significant convective flux penetrates above the rather artificial boundary separating the convection zone from the radiative equilibrium photosphere to heights where the radiative relaxation time has increased enough for the atmosphere to support gravity waves. Given a reasonably high efficiency for gravity wave generation (and this is predicted), it is still quite possible that the gravity wave flux might be as high as 10^6 ergs cm^{-2} sec^{-1} . Although no known dissipation mechanism makes these slow, low frequency waves a candidate for chromospheric heating, they must still be considered for coronal heating, where various 'frictional' and conductive processes may liberate the energy over a long path length, or where conversion to a different, hydromagnetic mode may occur. In addition, the possibility exists that the penetrative convection, in the presence of magnetic fields of 10 gauss or more in the low chromosphere, might give rise to torsional oscillations which propagate upward along magnetic lines of force, dissipating their energy by Joule heating of the atmosphere. Howe (1969) performed a linearized calculation and concluded that such a mechanism could account for spicules, although the conclusion is highly tentative and illustrates the difficulty of treating problems where medium compressibility, gravity, and magnetic field may all play a role.

It is safe to say that, while Whitaker's original ideas on gravity waves in the Sun have not stood up, the gravity mode and other modes generated by penetrative convection in the upper photosphere and low chromosphere are probably present, and that they may play an important role in heating both the corona and chromospheric, particularly in regions of magnetic field strength exceeding 10 gauss.

A discussion of waves in the chromosphere would be utterly incomplete without a consideration of the 300 sec velocity field oscillations which have actually been directly observed, in contrast to the high frequency sound waves, hydromagnetic modes, and gravity waves for which the evidence is, at best, more indirect. Ever since their chief characteristic features were first described by Leighton, Noyes, and Simon (1962), the question has been raised as to what role these oscillations might play in heating the outer atmosphere. Frazier (1968) obtained power spectra for both velocity and intensity fluctuations in three lines spanning the photosphere from the top of the convection zone to the temperature minimum, with sufficient resolution and observing time to break up the 300 sec oscillation into two, long duration, constant period velocity fluctuations of 265 sec and 345 sec. Furthermore, the amplitude ratio of

the short to the long period oscillation was found to grow with height. In addition, a strong, low frequency, convective component of the velocity field was found to persist right up to the temperature minimum. Finally, the duration of the velocity fluctuations suggested little or no correlation with the granulation. The implications of these and other observations analyzed during the past few years have stimulated a new round of theoretical activity which we are still experiencing right now.

It was immediately recognized that the granulation, which is our observational evidence for the turbulence which we believe generates the relatively high frequency acoustic spectrum studied by Stein, is in no direct way connected with the 300 sec oscillation, in contrast to the earlier notion that granule "pistons" might be driving them. Also, the observational evidence for penetrative convection at the temperature minimum kept alive the possibility that gravity waves might play a role in atmospheric heating, as already mentioned.

The most significant development to follow Frazier's work, however, in my opinion, is the two studies by Ulrich (1970) and Leibacher (1971), in which what seems to be a plausible mechanism for the 300 sec oscillations is discussed, and where the resulting eigenmodes are followed through much of the photosphere and chromosphere, where they begin to lose their energy rapidly through non-linear (shock) dissipation.

Ulrich's work concentrates on the generation of the oscillations; Leibacher's, on the propagation and dissipation. Both agree that the observed oscillations in the photosphere cannot be standing waves in the sense of running waves constructively interfering as they move back and forth between reflecting boundaries. The critical frequency for sound wave propagation is too high in this region, as we have already noted. In the absence of a forced, but decaying, oscillation pumped by the granulation, what are we really observing in the photosphere? Ulrich may have supplied the answer by recalling that small perturbations can lead to overstable oscillations in the presence of a superadiabatic temperature gradient in the presence of radiative cooling, a condition which is described by Moore and Spiegel (1966) and applies to the top of the solar hydrogen convection zone. Given this situation, Ulrich noted that the upper convection zone could trap standing acoustic waves, which would then drive the photosphere at the appropriate eigenfrequencies determined by the boundaries of the resonant cavity below. Although the waves could not propagate as running waves into the "forbidden" region around the temperature minimum, it is easy to show that the decay distance for the energy density $1/2 \rho v^2$ (v = material velocity) is quite long there. (The notion of reflection at the boundary follows from ray acoustics and is highly approximate here, as the ratio of the very long, > 1000 km,

wave length to scale height is quite large.) Detailed calculations show that attenuation is not too rapid. Indeed, the velocity amplitude actually increases with height in the atmosphere, so small is the density scale height.

The reason for the trapping follows readily from a cursory examination of the dispersion relationship for waves in a compressional atmosphere with gravity (again zero magnetic field for simplicity). It is necessary to apply this relationship, which follows, to a non-isothermal atmosphere such as the top part of the convection zone. The dispersion relationship is

$$k_z^2 = \frac{\omega^2 - \omega_s^2}{c_s^2} - k_x^2 \left(1 - \frac{\omega_g^2}{\omega^2} \right) \quad (1)$$

where all the quantities were defined in discussing Whitaker's work, except here,

$$\omega_g^2 = g \left(\frac{\gamma - 1}{c_s^2} g + \frac{1}{T} \frac{dT}{dz} \right)$$

should be used for the Vaisala-Brunt frequency in this non-thermal situation (cf. Kuperus, 1965). The lower boundary occurs where the inwardly increasing temperature decreases the first term on the right hand side of equation (1) so that, for a given finite (non-zero) value for the horizontal wave number k_x , it becomes equal to the second term, which will be of opposite sign for $\omega_g < \omega < \omega_s$, the frequency range in which the observed oscillations lie. Thus, $k_z = 0$ results, defining a lower reflecting boundary. The upper boundary occurs where the two terms again cancel, this time because, for a given ω , the outwardly decreasing temperature causes a correspondingly increasing ω_s to approach ω in value. The result is a resonant cavity for eigenmode (ω, k_x) , given a model for the upper convection zone and photosphere.

To actually obtain eigensolutions, one must, of course, solve the appropriate wave equation with boundary conditions which depend on the eigensolutions (ω, k_x) . Ulrich obtains a simple, workable, lower boundary condition from equation (1), by noting that $\omega_g \rightarrow 0$ as one goes into the convection zone. Then he determines the upper boundary by finding the

mode which has the smallest velocity amplitude above the temperature minimum, on the grounds that this mode should be distorted least by shock formation in the upper atmosphere and, thus, provide the most reliable boundary matching. His eigensolutions include a fundamental mode and first-overtone mode which pass through the peaks of Frazier's published power spectra. To establish that these oscillations are overstable, Ulrich is forced, by his method of handling the outer boundary condition, to consider the energy balance. When he does this, he finds that the fundamental mode and first two or three overtone modes are overstable. In addition, he estimates the outward energy flux in these oscillations is greater than 10^6 ergs $\text{cm}^{-2} \text{sec}^{-1}$, or roughly in agreement with estimated net radiative losses from the outer atmosphere reported by Athay (1966). Although I would take issue with his speculations as to what happens to the waves as they heat the outer atmosphere (conversion to heat through some hydromagnetic interaction), it seems to me that Ulrich has come closer than anyone, to date, to providing insight into the *origin* of the 300 sec oscillations. In addition, he concludes his article by outlining the kind of observations necessary to further check some of these ideas.

Leibacher, on the other hand, while concluding independently that the mechanism of subphotospheric standing waves is responsible for the observed photospheric oscillations, concentrates on the properties of the observed "evanescent" oscillations themselves. He shows how the evanescent waves become propagating waves once more, due to the chromospheric temperature rise, and calculates the atmospheric heating through non-linear dissipation. Further results which I'll mention in a more detailed treatment of the heating make this seem very plausible. That is, there is good reason to believe that 300 sec progressive waves will develop very quickly into strong shocks, so that complicated hydromagnetic interactions are unnecessary. Therefore, these interactions, mentioned by Ulrich would seem less likely to be important in heating the upper chromosphere or transition region, at least, outside of plages and spicules. The position of the evanescent waves in an isothermal temperature trough is shown on the diagnostic diagram of Figure III-3, which appears in Leibacher's thesis. We see immediately that their range of (ω, k_x) , which corresponds to observed values, is quite incompatible with propagating acoustic or gravity waves. They are on the other hand, completely compatible with the picture provided by the more recent work.

This concludes what I want to say about the 300 sec oscillations. There isn't time to review past theoretical efforts to understand them. Most of these efforts have run into serious objections, often as refined observations clarify what the Sun is doing. An earlier effort by Moore and

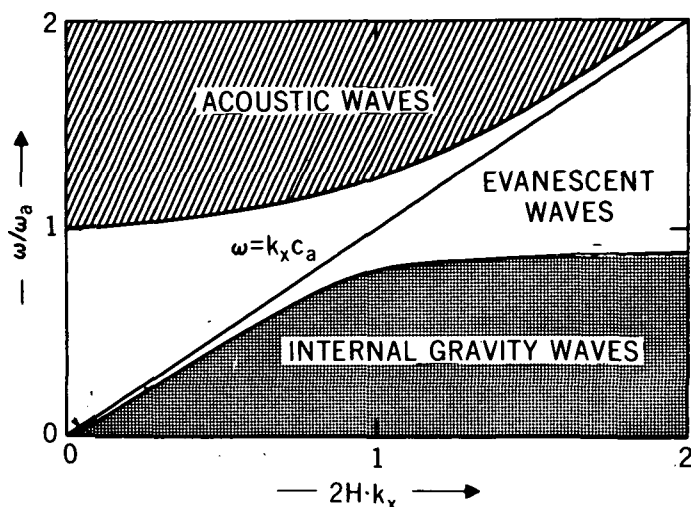


Figure III-3 Evanescence waves on solar diagnostic diagram.

Spiegle (1964) suggested the evanescent wave interpretation, which now seems promising, without offering the explanation of underlying standing waves. Time and better observations, particularly of phase relations in two dimensions, will permit us to check the more recent work of Ulrich and Leibacher.

SOLAR ATMOSPHERIC HEATING

Keeping all these remarks on wave modes in mind, I'd like to turn to the heating question. Since most of the quantitative work on this question has been restricted to the chromosphere, it is useful to start there and work up.

The earliest idea, already discussed, was that sound waves generated by turbulence at the top of the convection zone would build up into shock waves, as they propagate out into the sharp negative density gradient, and rapidly give up their energy, thus producing the abrupt transition to coronal temperatures and heating the corona itself. Recent detailed work (cf. Ulmschneider, 1970, 1971 a,b), using the theoretical acoustic spectra of Stein — Figure III-1 again — has modified the original picture in several ways.

By following the growth of the sound waves from their point of generation up through the photosphere and low chromosphere of a typical solar atmospheric model, Ulmschneider has shown that a fully developed shock wave (crest of an initially sinusoidal wave has caught up

with the trough) develops after the wave has traversed a few scale heights, i.e., several hundred kilometers. This particular conclusion is in substantial agreement with several earlier studies. The result is important in insuring that significant shock heating will occur around or slightly above the temperature minimum, where, as we shall see, some mechanical heating appears to be necessary. A departure from the original picture occurs, however, when Ulmschneider solves the weak shock propagation equation for these waves. He shows that, for the relatively high frequencies of the Stein acoustic spectra (typically 30 sec period), the shock Mach number remains small enough in the low chromosphere to preserve the validity of the theory; and this permits estimates of the local mechanical heating to be made by using it. He then calculates the heating in this way, and finds good agreement between the heating and the local net radiative losses due to H^- , which are computed using the same model. This is illustrated in Figure III-4. Earlier studies either ignored the situation in the low

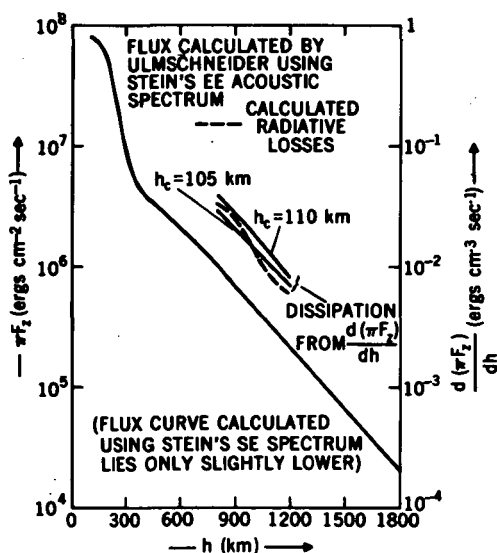


Figure III-4 Mechanical flux and dissipation in chromosphere

chromosphere or treated it very approximately. Furthermore, the earlier notion that the waves generated by the turbulent convection are responsible for the chromosphere-corona transition and the high coronal temperature now seems wrong. It is the low chromosphere, alone, below the sharp upward temperature transition, where these waves seem to be effective. Higher up, we appear to need the 300 sec progressive waves and, possibly, other modes.

The importance of Ulmschneider's results can best be seen, I feel, if we keep two things in mind. First, it is useful to recall that, if the low solar chromosphere does require mechanical heating, as now seems well established (Athay 1970), the net radiative losses from this region of almost negligible extent (compared to, say, the corona) are probably equal to the sum of all the other net radiative losses from all other sources in the entire outer atmosphere beyond the temperature minimum. This is due, of course, to the relatively high densities in the chromosphere compared to the corona, notwithstanding the much higher coronal temperature. This observation, though reported often, does not seem to have made much impression on some astronomers who talk about the heating problem as if coronal heating were the sum of it. Obviously, a region, however small, is fundamentally important if (1) much of the heating must, ultimately, occur there, and if (2) the waves responsible for heating all the higher regions must pass through it. Incidentally, this problem of energy balance in the chromosphere is a principle reason for energetic efforts to determine, from observations, the optical depth, breadth, and value of the minimum temperature. These efforts, which sometimes involve considerable expense—for high altitude infrared observations, for example—are certainly worthwhile.

Consequently, Ulmschneider's rather satisfactory treatment of the low chromosphere has importance in its own right. Looking ahead, it keeps alive the hope, already mentioned, that a relatively simple heating theory may be applicable to building one-dimensional non-radiative equilibrium atmospheric models for a large class of late type stars with convective envelopes.

This brings us to the upper chromosphere and/or the transition region.* What causes it? This is certainly still an unanswered question, but recent work on shock theory offers one interesting possibility in the magnetic field free regions. Several recent calculations show that the relatively low frequency waves associated with 300 sec oscillations will develop into strong shocks in the upper chromosphere, and the sudden release of a large burst of energy in this way could cause the transition to coronal temperatures, if the atmosphere cannot lose the energy over a shocking cycle under chromospheric conditions (Jordan, 1970). This mechanism raises as many questions as it attempts to answer and says nothing about the complex spicule phenomenon, but it has the merit of simplicity and, recently, some additional support, both from the theoretical picture of the 300 sec oscillations and how they develop when they become progressive waves, as well as from some recent observations from the

*I'll use these two terms interchangeably. Usage varies.

OSO-7 satellite (Chapman et al., 1972). These satellite data give evidence for periodic changes in upper transition region conditions, as inferred from approximately 300 sec periodic changes in intensities of lines from He II, Mg VIII, and Mg IX. These changes could be caused by periodic temperature fluctuations due to strong shock waves passing through this region, consistent with Leibacher's theoretical calculations.

One of the serious problems that the strong shock hypothesis runs into is refraction and, to a somewhat lesser extent, reflection from the sharp temperature rise. These effects could reduce the outward flux in these waves below the value required to balance energy losses in the corona. Even more to the point here, the sharp temperature rise implies a strong conductive flux from the corona back down into the chromosphere. All of these processes will be further complicated where there are magnetic fields.

These complications do not preclude shock heating in the transition region, but they do show that the total heating picture is probably much more involved. In particular, until we have a reliable observationally determined temperature model of the transition region, it will be difficult to determine the conductive flux at various points and, hence, the conductive heating. One real hope for progress soon is that planned high resolution satellite spectra in transition region lines will provide us a sufficiently good model to permit the shock heating and conductive heating calculations to be made there. Then we can not only discriminate better among various possible transition region heating modes, but also determine better what waves can continue on into the corona.

One summary picture of solar chromospheric heating, consistent with the work reported and restricted to that great bulk of gas for which the magnetic field is negligible ($\lesssim 10$ gauss), might appear as follows: Sound waves are generated by turbulent convection in the low photosphere and, thanks to their comparatively high frequencies, they pass through the temperature trough and develop quickly into weak shock waves. As such, they deliver their energy to the low chromosphere, balancing the net radiative losses in H^- and a number of medium to strong spectral lines, and then pass into the transition region where their behavior is less well known, but their residual energy flux, and hence their effect, is small, perhaps negligible. On the other hand, the 300 sec periodic oscillations in the temperature trough have been transformed, by the outward rise in low chromospheric temperature, from non-propagating, evanescent waves into progressive sound waves and develop quickly into strong shocks, capable of producing a rapid temperature rise by heating the gas (ionizing hydrogen) beyond its capacity to remain thermally stable at low chromospheric temperatures. A significant conductive flux back-down will result

from this rapid temperature rise, and the heating associated with this flux will, along with the strong shock heating and the radiative cooling, determine the final temperature structure and energy balance of the transition region.

Since this summary picture is necessarily tentative, it might be useful to mention several critiques of the above ideas. I'll then indicate why the above picture still seems the most compelling to me.

First, we cannot discount completely the possibility that the temperature rise in the low chromosphere is produced in radiative equilibrium, eliminating the need for mechanical heating there (Cayrel, 1963). Some of us, including myself, felt that this idea was fundamentally incompatible with the non-LTE situation in the H^- continuum, but this proved to be wrong, due to the non-coherence of the continuum scattering (Skumanich, 1970). Thus, it was evident that only detailed calculations could settle this issue. In particular, given a reasonable density distribution for the chromosphere, and the effects of line blanketing on the temperature there, the question becomes: will a radiative equilibrium, blanketed model exhibit temperatures as high as those obtained from current observationally determined models. Athay, (1970) did this calculation and concluded that, although no mechanical heating would be needed to produce a temperature minimum of 4400° K at τ_5 (normal optical depth at $5000 \text{ \AA} = 10^{-4}$, mechanical energy would be required above this point. This agrees with a calculation I have done, using Athay's blanketing functions and a formulation of the problem similar to Gebbie and Thomas (1970). At this stage, it appears that the cooling due to line blanketing above the temperature minimum more than offsets the tendency of the non-LTE Cayrel mechanism to increase the temperature. Consequently, mechanical heating will be necessary to produce a temperature rise in the low solar chromosphere.

I might mention here a subject I am not competent to evaluate, but one which is very important. This is the possibility of radiative equilibrium temperature rises in early type stars, discussed briefly in Mihalas (1970) and, in greater detail, in a series of papers by Mihalas and Auer which appeared in the *Astrophysical Journal* over the late 1960's. If this rise occurs in radiative equilibrium, up to the color temperature of the background continuum (otherwise, the second law of thermodynamics is violated), this could reduce the requirements for mechanical heating significantly. Finding a source of mechanical energy is a serious problem for these hot, early type stars, as they have radiative, not convective, subphotospheric envelopes.

Another possibility for the solar chromosphere, advanced by one of the participants, is the suggestion by Ulrich (1972) that radiative dissipation

of sound waves might produce the temperature rise. Ulrich questions the shock hypothesis on the grounds that evidence for the waves is lacking, but it is not obvious that we have taken the observations or properly analyzed the data to confirm or rule out the shocks. Quite to the contrary, this is the object of several current research programs. It is probably premature to judge the radiative damping mechanism, which depends strongly on such parameters as wave frequency, radiative relaxation time (hence, non-LTE effects), and material velocity in the chromosphere. Nevertheless, given the sharp negative density gradients in the low chromosphere, and considering the granulation evidence for a turbulent region in which the necessary high frequency sound waves can be generated, not to mention the results of weak shock calculations, it would seem that the shock heating mechanism still offers the most natural way to heat the low chromosphere.

Subject to these alternate possibilities, the shock heating picture looks very promising. In view of this, it might be worthwhile pointing out what form of the weak shock theory is valid for chromospheric calculations, where the Mach number does not greatly exceed unity. Some conflicting results have appeared in the literature, and it is now clear how this conflict arose.

Osterbrock (1961) is the first person to publish an application of what we call weak shock theory to the chromospheric heating problem, to estimate mechanical heating as a function of height for a given temperature-density model. As we have seen, his conclusion that weak shocks probably heat the low chromosphere seems as likely today as it did then. On the other hand, much else has changed, and it is somewhat ironical that this original conclusion still stands. First, current chromospheric models have a much smaller density scale height than the van de Hulst (1953) model used by Osterbrock. Second, we now believe that wave periods around 30 sec are more apt to characterize the turbulence generated sound than the 100-300 sec range used prior to Stein's (1968) work. Third, it is easy to show that, for these short period waves in the chromosphere, the approximation used by Osterbrock to evaluate the mechanical flux integral leads to serious over-estimates in computing the growth of the shock strength and the dissipation.

Ulmschneider, in the studies referenced earlier, has performed the evaluation correctly, provided the shock is truly weak. Such a weak shock is represented by a $P(t)$ curve calculated by Schwartz and Stein (1972) for an initially sinusoidal disturbance of period 100 sec under low chromosphere conditions. The $P(t)$ relation behind the shock front is almost linear. This linear relation is equivalent to assuming that the relaxation phase of the wave's passage can be represented by a simple wave in a

perfect gas (cf. Landau and Litshitz, 1959, p. 367). This is not unreasonable if the entropy change during the relaxation phase is not too abrupt (in marked contrast to the initial "shocking" phase). So by assuming a linear $P(t)$ relation over a shocking cycle, one can evaluate analytically the mechanical flux integral

$$\pi F_+ (\text{mech}) = \frac{1}{T} \int_0^T (P(t) - P_0) u(t) dt, \quad (2)$$

where P_0 is $P(t=0)$ and T (here) is the period, for a given, simple rest frame velocity $u(t)$, usually chosen to be a sawtooth N-wave. In fact, it can be shown that the result of integration is almost independent of the ratio of the velocity relaxation time to the period, as long as this ratio does not become much smaller than $1/3$. Using the resulting expression for $\pi F_+ (\text{mech})$ in the shock propagation equation, it can be solved for a given atmospheric model. This is what Ulmschneider did. His results confirm Osterbrock's original conjecture, but only because the tendency of new, small scale height models to cause explosive growth of the shock is offset by the shorter period and less approximate method for evaluating the mechanical flux integral. We have come full circle in a decade.

The work of Schwartz and Stein, just mentioned, and its antecedent (Stein and Schwartz, 1972) bear directly on this question of ranges of validity for the weak shock theory. They show that, as expected, for a relatively short period wave (100 sec vs. 400 sec), where weak shock theory begins to become applicable, a careful treatment of the growth of the initially sinusoidal disturbance is necessary to prevent an overestimate of the heating low in the atmosphere, and the weak shock theory will seriously underestimate the heating as the Mach number approaches 2. Fortunately, Ulmschneider's calculations exhibit a lower Mach number throughout the low chromosphere.

It seems that periods of around 100 sec (corresponding to roughly twice the acoustic cutoff frequency ω_a) represent the upper limit for a weak shock treatment of chromospheric waves. Figure III-5 shows the results of a calculation I did, using the Harvard Smithsonian Reference Atmosphere (Gingerich, Noyes, and Kalkofen, 1971) and solving the shock propagation equation exactly as Ulmschneider did. We see that, for a 30 sec shock, the shock strength parameter η remains almost constant with height as Ulmschneider concluded. For a 95 sec shock (the velocity relaxation time τ_0 differs by a negligible amount here — it was varied during the calculation), η grows rapidly with height and eventually exceeds the range for validity of the weak shock theory, thus yielding spurious values for the

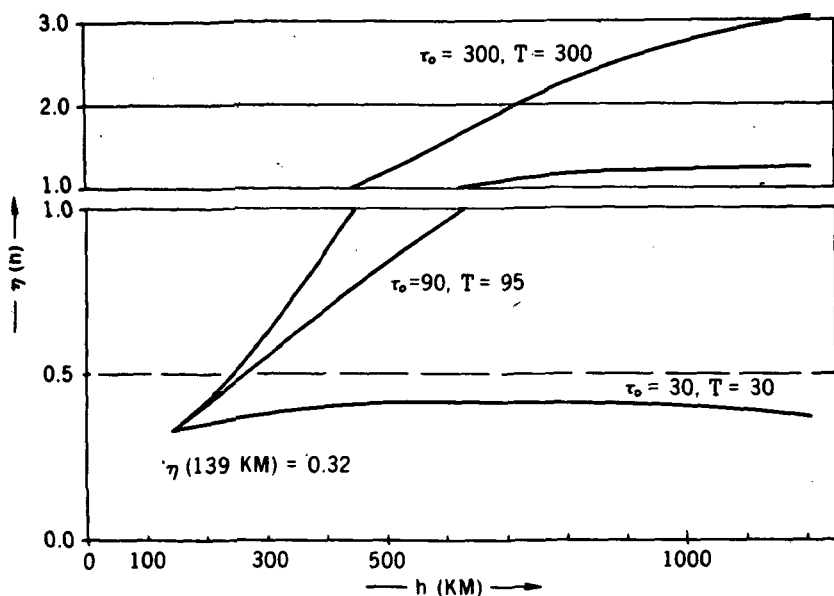


Figure III-5 Shock strength parameter $\eta(h)$ vs. h for HSRA model and different periods τ (sec).

dissipation, as noted by Schwartz and Stein. Finally, for a 300 sec shock, the wave, once assumed to be fully developed, grows explosively, and cannot be treated by the weak shock method, consistent with the previous work of all of us.

This concludes a survey of the situation in the chromosphere, including the transition region, and brings us into the solar corona. What heats the corona? We don't know. It's even hard to make an educated guess, because there are problems with all the wave modes proposed.

The Alfvén mode is the favorite candidate of a number of authors, for several reasons. First, one important effect of a magnetic field will be to couple the different wave modes in the chromosphere, leading to a transfer of energy from the fast mode (which, you will recall, is just a sound wave in a zero magnetic field) into the Alfvén mode, in regions where the Alfvén speed exceeds the sound speed. Since the Alfvén speed is given by $c_A = B/\sqrt{4\pi\rho}$, and since density drops off faster than temperature increases up to the transition region (or, more to the point, $c_A \uparrow$ faster than $c_s \uparrow$ as $h \uparrow$), we see that this situation will exist everywhere in the chromosphere where $B \gtrsim 10$ gauss. The Alfvén mode has the right propagation properties for coronal heating too; namely, it can penetrate to the corona without appreciable dissipation. This is largely due to the non-compressible feature of the Alfvén wave, which will follow magnetic field lines up into the corona. The problem is that no one, to my

knowledge, has offered a satisfactory dissipation mechanism for these waves in coronal gas, whose low densities appear to make the various collisional mechanisms inefficient.

A similar problem exists for the gravity mode, which Frazier's (1968) observations suggest should be present due to the presence of penetrative convection near the temperature minimum. Again, how is the energy dissipated in the low density corona? The long wavelength and low frequency gravity wave does not lend itself to shock dissipation there, and linear dissipation processes appear too inefficient.

One useful bit of information bearing on this problem would be to determine, once and for all, if the quiet solar corona, observed at sunspot minimum, is a phenomenon of *only* regions of significant magnetic field strength, with material at essentially interplanetary densities between the magnetic regions (cf. Billings, 1966, Chapter 3.). If this proves true, it would restrict our search to waves and heating mechanisms effective in these regions. In particular, it would favor the Alfvén wave hypothesis, or, perhaps, the one proposed by Howe (1969) and mentioned earlier over heating by ordinary gravity waves.

A popular hypothesis over the years has been that the progressive waves generated near the top of the convection zone heat the corona by shock dissipation. This raises just the opposite problem from the Alfvén and gravity modes. Dissipation by shocking could heat the gas, but getting these progressive waves into the corona with an adequate energy flux looks difficult. The high frequency sound waves which are likely to heat the low chromosphere dissipate practically all of their energy there, according to all our recent calculations, which are of course, model dependent. The 300 sec waves may carry sufficient energy to the base of the transition region, but refraction and reflection off the sharp temperature rise probably reduce this flux several orders of magnitude, so, while these waves can easily heat the transition region right up to the 10^6 °K corona, they may not have sufficient vertical flux to balance the various coronal losses. Again, this conclusion is model dependent, and could change as we get better models for the transition region.

CONCLUSION

It should be evident from these remarks that one of the crucial theoretical problems is the behavior of a system of waves under chromospheric conditions in the presence of a magnetic field. How do they interact with the medium and with each other? What new modes appear as a result of this interaction? Frisch (1964) has addressed himself to this problem, which involves some unpleasant non-linearities, and finds that

with a WKB approximation the rotation of the magnetic field couples the modes. Stein and Uchida, among others, are working on the problem now, and many of us await their results eagerly.

I'll close this survey of solar atmospheric heating on the optimistic note that, thanks to the high spacial resolution possible on currently flying and planned future solar satellites, coupled with good time and spectral resolution, we can confidently expect to learn much more about oscillatory velocity fields and general chromospheric and coronal structure in the 1970's. The two pointed experiments on OSO-I, scheduled for an early 1974 launch, will obtain simultaneous spectra in a large number of uv lines, with spacial resolution approaching 1 arc sec, time resolution of 10 sec, and spectral resolution of $.05 \text{ \AA}$ or better. This will permit us to do many things, like testing the chromosphere for the presence of high frequency waves in the region where the core of the strong MgII resonance doublet is formed. This is the very region where we expect strong dissipation from these waves.

For those of you interested mainly in non-solar stars, I hope this review has demonstrated two things: (1) The shock dissipation hypothesis still seems the most attractive for the Sun, outside of, possibly, the corona. (2) Nevertheless, there are still other candidates for the heating, so great caution must be exercised in treating chromospheric/coronal heating of non-solar stars with strong convective envelopes by some shock dissipation theory.

Several efforts have been made to treat late-type stellar atmospheres in this spirit over the past decade. In this afternoon's discussion, I'll attempt a critique of one of the latest and most comprehensive of these studies.

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DISCUSSION FOLLOWING THE INTRODUCTORY TALK BY JORDAN

Skumanich — I would like to ask a question about the zeroth order atmosphere for which you are doing the calculation of this heating. Do you start with the models that we radiative transfer types give you?

Jordan — Yes. The calculations in my talk were done for a number of models including a current version of the Harvard-Smithsonian Reference

Atmosphere, which is, I think, the best current model. In general, one sees that the results for the heating are almost model independent, as the crucial parameter, the scale height, is not strongly model dependent.

Skumanich — Keeping in mind that these are average models, where do we look to better understand heating in the light of these theories, in the network or in the cells?

Jordan — We look in the cells. What is really interesting, however, is the fact that when we calculate mechanical dissipation rates with the weak shock theory in the low chromosphere, using these average models, and compare the results with computed values for the net radiative loss due to H^+ , or even make some rough approximation to the blanketing by using the Athay-Skumanich blanketing functions, we get surprisingly good agreement. I think that this is good evidence that the weak-shock theory is a good first approximation theory for the heating in the low chromosphere.

Beckers — In connection with the observation, several years ago, I took observations in the K and H lines on the disk with a time resolution of 5 sec and a spatial resolution of one or two arc sec. I never saw any periodic phenomena—varying with periods less than 100 seconds.

Ulrich — In your relation between pressure and velocity, did you include the effect of radiative dissipation?

Jordan — No.

Ulrich — As I shall discuss later, this could be important.

Stein — Would you really expect to see waves of such high frequency, since the spectral lines you used to study the oscillations are formed over a certain atmospheric depth? The velocity profile goes from maximum to minimum over a period in a nearly linear way, or the variation is slow, whereas the pressure goes from maximum to minimum rather steeply. The question is, then: Is the perturbed atmospheric region small compared to the region over which the line is formed? Can the oscillation even be detected?

Athay — I've computed the width of the contribution function in the chromosphere for the K line for a region making about equal contribution to the intensity. It comes out to 300-400 km. This is the same order as the wave length you are talking about.

Skumanich — But the velocity field is going to be weighted most heavily by the emission at the head of the wave, so it's not a simple question.

Jordan — And you must keep in mind that high time resolution is necessary if there is to be any hope at all, as the time of a single

observation must be short compared to a wave period or we won't see any periodic variations. Both high time resolution and a careful analysis will be necessary to settle this question.

Sheely — I'd just like to point out that we do have data to answer some of these questions. Time resolutions of 5, 10, 15 sec and high spatial resolution in a great number of lines: *H α* , the K line, etc.

Thomas — I'd like you to clarify once again what region of the atmosphere most of your remarks pertain to?

Jordan — The cell. The non-magnetic chromosphere above the supergranulation element. Not the sunspot. Not the plage. Not the spicule.

Thomas — Why do you restrict yourself to this region?

Jordan — Because there is where we think the bulk of the chromospheric gas is located. The good correspondence between calculated dissipation and net radiative losses as a function of height throughout this low chromospheric region suggests that these simple, one-dimensional models which ignore the magnetic fields may not be too bad. Thus, though we admit, or at least I do, that we can't do the heating calculation in the presence of magnetic fields yet, this may not be too serious for the solar chromosphere.

Skumanich — In doing this you're avoiding the coronal heating problem.

Jordan — More. You're avoiding the role of the transition region, which could produce a large conductive flux down. This could be serious.

Skumanich — I'm worried about the fact that, in the results you showed, the shock strength parameter becomes uncomfortably close to unity. I recall the value $1/3$.

Jordan — But $1/3$ is not uncomfortably close to one in this theory. First, the coefficients of the higher order terms are very small. More reassuring, laboratory experiments show that the theory is very accurate in this Mach number range.

Frisch — Why is the knowledge of the temperature structure not sufficient to determine the conductive flux?

Jordan — If we write down the usual expression used to compute conductive flux, where this flux varies as the $5/2$ power of the temperature, you might think that all we had to do was to differentiate this expression to get the heating; but that's not necessarily true. We don't know the value of the coefficient, which depends upon, among other things, the magnitude and direction of the magnetic fields.

Skumanich — But we know about these fields in the network.

Jordan — But that's not the region we're talking about. What about strong, as yet unobserved, horizontal magnetic fields over the cells, above where the weak shock heating occurs, yet in the transition region of strong conductive flux? This is a real possibility.

Jefferies — You went over coronal heating rather swiftly. What seems to be the essence of the problem?

Jordan — Among other things, I don't think we really know what wave modes exist in the corona. There may be some who would take issue with that statement, but if you accept it, then you can see that it would be rather meaningless to estimate the heating theoretically. Estimates based on observations have been offered, of course, equating necessary heating to net radiative losses, conductive losses down, and convective losses out.

Stein — I think the problem goes deeper than that. I believe that in the near future we'll be able to say what wave modes exist in the corona. But there is the further problem that the total amount of energy needed to heat the corona is small compared to the total energy in the waves when they are generated lower down. When you consider the errors inherent in estimating the energy generated, the dissipation lower down, and the energy in waves produced by wave-wave interactions, you find that these errors are of the same order as the amount you need to heat the corona.

Thomas — Are you saying that most of the energy of these waves is lost before they reach the corona? I'm not sure of the picture.

Stein — All I'm saying is that estimates of the amount of energy needed to balance coronal radiative losses, conductive flux, and the solar wind are small compared to the amount originally generated. We can estimate the amount of energy in the 300 sec oscillations, for example, and then when we consider the errors in this estimate, they might be of the same order as the amount of heat needed in the corona.

Skumanich — I think you fluid mechanics people are avoiding the question of reproducing the dissipation that can be inferred from the temperatures which we spectroscopists derive for the corona and the transition region. The real problem is that you are unable, with your theories, to predict the observed flux divergences high enough in the atmosphere that are inferred from spectroscopically determined temperature distributions.

Schwartz — But you're talking about the difference between two very large numbers, and this difference can be very small.

Thomas — If I understand the picture correctly, we have not one, but two competing mechanisms operating here in the low corona just above

the transition region. In addition to the conductive flux down, we have also the convection outward, both in that region where mechanical heating due to some mechanism is taking place. This is a more complex picture than the one you're talking about Andy.

Skumanich — That's right.

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THEORETICAL UNDERSTANDING OF CHROMOSPHERIC INHOMOGENEITIES

Philippe Delache
Observatoire de Nice

"By assuming that the atmosphere is homogeneous at each depth, we are immeasurably adding to the numerical tractability of the problem at the expense of ignoring 80 years' worth of data on chromospheric inhomogeneities"

LINSKY and AVRETT (1970)

INTRODUCTION

To the spatial inhomogeneity, Linsky and Avrett could have added the variations with time which are also well known, well observed characteristics of the solar chromosphere. Let me quote also Praderie (1969): large asymmetries are observed in stellar K_2 components which vary with time, "so that it seems difficult to think of any interpretation of the K line profile that would ignore motions and inhomogeneities in the atmosphere of those stars". And let me borrow a conclusion from Thomas (1969): "So what we need are ingenious ideas for empirical inference; or theoretical generalization from experience with the solar case". I wonder if the solar experience is sufficient at the present time to permit any theoretical generalization, as has been the case for the solar wind. In order to simplify, I shall restrict the scope of this contribution to the quiet solar chromosphere, and focus only on spicules. It is quite possible that, in ignoring plages and active phenomena, we miss an important clue to the understanding of inhomogeneous structures. But we also have to "add to the tractability of the problem".

Now, one *basic observed property* of the solar chromosphere is undoubtedly its inhomogeneous structure; at the present time, the *basic physical property* seems to be the mechanical energy deposited. So a first question could be: how fundamental is the relation between mechanical energy deposition and inhomogeneities? The answer is not clear, since the way is very long which has to go from the origin of mechanical energy, its transport (or propagation), its deposition, its effect on the state parameters, on the macroscopic structures, and then the prediction of escaping

radiation, which is what we observe. We must not forget that we have at our disposal numerous studies where the inhomogeneous structure is an essential starting point (or conclusion) together with completely homogeneous theories, some of which are successful. Our question could then be replaced by the following: in neglecting the temporal and spatial factors, do we lose a significant amount of the physics? and how complicated would it be to include the (t, r) parameters in the existing theories?

The chromosphere-corona transition region should obviously be included in our study, since its structure is continuously connected to the chromosphere. This continuity, essentially with respect to mass flow, has been stressed by Zirker (1971).

In the following, we shall start from the observations. As we shall see, it has been possible to infer from them some empirical models, in which, very often, a great many theoretical considerations are embedded; generally, the transfer problems are partially solved, whereas the dynamical equations are not considered. I shall call this type of approach "descriptive theories".

Then we shall consider the mechanisms of some dynamical models that have been proposed to explain the machinery which is responsible for inhomogeneities.

After having stressed that, with little effort, we have at our disposal some simple tools for studying inhomogeneities, I shall give a brief account of a recent work in which the inhomogeneous structure of the chromosphere-corona transition region shows up very simply, from dynamical considerations applied to observations averaged over the whole disk of the Sun.

OBSERVATIONS – DESCRIPTIVE THEORIES

Spicules can be seen on the limb, and also on the disk, even if there still exists some disagreement on the correct detailed identification. They form families (brushes, coarse mottles) lying at the boundary of the supergranulation cells, where the magnetic field is known to be relatively strong. Most of the available information on spicules can be found in the very extensive survey made by Beckers (1968). More recent observations, essentially pertaining to the H and K problem, have been made with high resolution (spatial, temporal, spectral); for example by Bappu and Sivaraman (1971) who propose that the boundary of the supergranulation should obey the Wilson-Bappu relationship. It is possible to construct simple models for individual spicules and for the chromospheric background (sometimes called "interspicular" matter). As Zirin and Dietz (1963) mentioned, this kind of descriptive model may account for the

observations, but generally it does not answer the fundamental questions: what is the heating mechanism, and what makes spicules? Recently Krat and Krat (1971) deduced that the classical model of a rotating spicule made of a Ca II core with a Helium envelope is still adequate for the interpretation of their high spatial resolution observations in H α , H β , D $_3$, H and K. The question of the dynamical state of such a structure is avoided in saying simply that it is compatible with the model of Kuperus and Athay (1967). Going to the chromosphere-corona transition region, Withbr e (1971) also gave a crude description of a spicular structure that is needed to explain center-to-limb XUV observations. Beckers (1968) also gave a descriptive model of a two component chromosphere, and very carefully made warnings on the validity of such an approach. First, he obtains a pressure inversion in the interstellar region. (Note that Delache (1969) has given a possible interpretation in terms of momentum transported by the heating waves.) Second, he questions the validity of a statistically steady state; as an example, the recombination time for a proton and an electron ($T_e = 15\,000^\circ$, $n_e = 10^{11}\text{ cm}^{-3}$) to the first and second level is 1.0 or 2.5 min respectively. Similarly the quasi-static behaviour of the radiation field could also be questioned. The random walk of a photon in an optically thick spicule can take a long time! Preliminary work shows that the process can be described in a diffusion approximation (Delache, Froeschle, 1972; Le Guet, 1972).

Since, clearly, one cannot avoid going to the dynamical models, let me list some observational requirements, as given by Beckers (1968):

- A spicule moves up ($\cong 25\text{ km s}^{-1}$), slows down, and approaches a standstill; "it is likely that it returns to the photosphere after it becomes invisible".
- At two different heights, the accelerations are practically simultaneous: the accelerating force propagates with velocity $v > 500\text{ km sec}^{-1}$.
- Spicules appear in the magnetic regions which outline the solar supergranulation ($B \approx 25\text{-}50\text{ gauss}$).
- Spicule diameters, birth rates, and lifetimes are similar to that of the granulation.
- Temperature T_e is nearly constant above 2000 km .
- Before the death of a spicule, its diameter increases.
- Left and right hand sides of a spicule are different, possibly indicating a rotation.

SOME DYNAMICAL MODELS

The first step in trying to put another kind of physics, besides just radiative transfer, into what I have called descriptive theories is, of course, to look at the energy problem. Thus, the various dynamical theories differ essentially in the heat supply. If mechanical energy is deposited in an inhomogeneous, time dependent pattern, this can be due to either (or both) of two reasons:

- The amount of energy available for absorption depends on \underline{r} and t .
- The process by which the energy is absorbed depends on \underline{r} , t .

In both cases, the currently accepted heating mechanisms can be responsible for the spicular structure; some of them have been studied in the homogeneous case, like shock wave dissipation, or heat conduction, together with the departure from radiative equilibrium. A recent review by Frisch (1972) describes the results obtained in coupling the heating mechanism with the radiation field in a stratified atmosphere. This kind of mechanical energy may, or may not, be available in an inhomogeneous pattern. For example, Kuperus and Athay (1967) propose that spicules be driven by the conductive heat flux. The latter is inhomogeneous from the very beginning due to the magnetic structure of the transition regions. On the contrary, Defouw (1970) describes a local instability sensitive to the magnetic field, which borrows the energy from a constant homogeneous source.

Other types of energy sources have been described which are basically inhomogeneous, as the kinetic energy of horizontal motion in the supergranulation, or the Petschek mechanism of magnetic line reconnection, as proposed in a qualitative manner by Pikel'ner (1971). As there is no reason why the starting inhomogeneities would be similar to one another, it is hard to see why the resulting spicules are so alike. However, the role of local parameters in fixing the \underline{r} , t properties of the dissipation are not excluded, and again, it seems worthwhile to study in some detail the "local machinery" that may lead to a relaxation, or unstable situation.

For Kuperus and Athay (1967), as we have said, the heat conducted backward from the corona in the steep temperature gradient of the transition region is responsible for the onset of a Rayleigh-Taylor instability. The authors describe the instability as caused by the upward pressure force in the dense layer, replacing the downward gravitational field of the classical instability. The quantitative analysis is missing; in fact Defouw (1970a) concluded their picture would lead to a stable situation.

In his paper, Defouw describes "thermal instability" but does not deal with the real heating mechanism. He assumes simply that there exists a heat loss function \mathcal{L} (energy loss minus energy gain per unit mass per unit time). The rate of energy input is assumed to be constant. Then, the instability is described. The initial idea goes back to Thomas and Athay (1961): if the hydrogen plasma is heated, it may become less and less able to get rid of its internal energy by radiation. Defouw finds that, depending on the temperature range, the temperature gradient, and the value of the density, one can have unstable situations. The presence of magnetic fields reinforces the instability. Growth rates, temperatures, and electron densities are in satisfactory agreement with the spicule observation. However, the radiation field is treated in the quasi-static, effectively thin approximation, and the energy supply is left unspecified.

At this point, I would like to make a general comment on "descriptive" and "dynamical" models, which comes from the coronal experience.

If one takes into account the energy equation, and the hydrostatic equilibrium for a fully ionized plasma, one can predict a static spherically symmetric solar corona (Chapman, 1959). One needs only to specify T_e , n_e at a boundary point, e.g., at the base of the corona. But this corona has a finite pressure far away from the Sun; one needs an artificial wall to sustain it. Once the wall is removed, the static corona is no longer stable. Is it going to show relaxation into inhomogeneous structures? This seems to be a very complicated idea. One has only to allow for a spherically symmetrical expansion; we add the mass conservation equation and wait for the steady state to establish itself. We do not have to impose any further physical boundary condition. In particular, the velocity v at our boundary point is fixed. The solution (Parker, 1965) is thus viewed as the asymptotic behaviour of a time dependent problem.

Thus, precisely because we think that the chromosphere can be locally unstable, the mass motion should be taken into account from the very beginning. In a following paragraph we shall see how this simple principle can yield to some interesting ideas in the chromosphere-corona transition region, possible connected with spicular structure.

SOME TOOLS FOR THEORETICAL STUDIES IN CHROMOSPHERIC INHOMOGENEITIES

In this section I would like to show, with three examples, that the tools that we need to begin are available or can be found with little effort in the existing literature.

FIRST EXAMPLE:

Local description of the instability condition by Defouw: after some calculations, one finds that a necessary criterion for instability is:

$$\Delta = \left(\mathcal{L}_x - \frac{\rho}{1+x} \mathcal{L}_\rho \right) \left(\mathcal{T}_\tau - \frac{\rho}{\tau} \mathcal{T}_\rho \right) - \left(\mathcal{L}_\tau - \frac{\rho}{\tau} \mathcal{L}_\rho \right) \left(\mathcal{T}_x - \frac{\rho}{1+x} \mathcal{T}_\rho \right) < 0$$

(\mathcal{L} is the heat loss function, \mathcal{T} is the number of ionizations per unit mass per unit time, ρ , τ , x are the density, temperature, ionization degree, and \mathcal{L}_x stands for $\frac{\partial \mathcal{L}}{\partial x}$, etc.)

This result has a simple local physical interpretation. In the equilibrium state, a given mass element has well defined energy E , number of particles N , and volume V . This reads:

$$E = \text{cst} \quad \rightarrow \quad \mathcal{L}(x, \rho, T) = 0$$

$$N = \text{cst} \quad \rightarrow \quad \mathcal{T}(x, \rho, T) = 0$$

$$V = \text{cst} \quad \rightarrow \quad P(x, \rho, T) = P_{\text{ext}}$$

(P is the pressure of the mass element, P_{ext} is the "external" pressure.)

What is the condition for the existence of an equilibrium (neutrally stable) x , ρ , T ? (Which is the starting point for a discussion of thermal instability, as in Souffrin, 1971.)

The answer is straightforward: $\delta \mathcal{L} = \delta \mathcal{T} = \delta P = 0$, i.e.

$$\mathcal{L}_x \delta x + \mathcal{L}_\rho \delta \rho + \mathcal{L}_T \delta T = 0$$

$$\mathcal{T}_x \delta x + \mathcal{T}_\rho \delta \rho + \mathcal{T}_T \delta T = 0$$

$$P_{\text{ext}} \left[\frac{\delta x}{1+x} + \frac{\delta \rho}{\rho} + \frac{\delta T}{T} \right] = 0$$

(since $P \propto (1+x) \rho T$).

A solution for δx , $\delta \rho$, δT different from zero can be found only if $\Delta = 0$. Thus $\Delta = 0$ is the condition for marginal stability. A closer examination will show which side has the instability. (*)

This does not mean that the complete calculation made by Defouw is useless. On the contrary, it is really necessary for a detailed description. This was intended simply to show that it is often possible to extract simple descriptions imbedded in stratified geometries or abstract calculations. These simple descriptions can be more than qualitative and can give valuable support to the intuition.

SECOND EXAMPLE:

This example is non-local, and mixes the heating process together with radiative transfer. Frisch (1970, 1971) has solved numerically the problem of radiative and conductive coupled transports in a stratified atmosphere. In her results, there seem to appear two regions; as a matter of fact it has been shown by Cess (1972) that an approximate solution can be found analytically within the framework of singular perturbations; the boundary layer can be treated separately from the interior. Again, from detailed results, it has been possible to infer an approximate, but much simpler,

(*) Note added in the final manuscript after a remark by R. J. Defouw.

The question is not really very simple: for example Defouw (1970b) interprets the procedure in the following way: Suppose that $\delta T = \delta P = 0$ and we calculate $\delta \mathcal{L}$ as a function of δT .

$$\delta \mathcal{L} = \frac{-\Delta}{\mathcal{J}_x - \frac{\rho}{1+x} \mathcal{J}_\rho} \delta T,$$

as $\mathcal{J}_x < 0$, $\mathcal{J}_\rho = 0$, the thermal instability criterion $\frac{\delta \mathcal{L}}{\delta T} < 0$ is equivalent to $\Delta < 0$.

One can object that it also seems legitimate to calculate $\delta \mathcal{J}$ as a function of δx if $\delta \mathcal{L} = \delta \rho = 0$.

Then

$$\delta \mathcal{J} = \frac{-\Delta}{\mathcal{L}_\tau - \frac{\rho}{T} \mathcal{L}_\rho} \delta x,$$

as $\mathcal{L}_q < 0$, $\mathcal{L}_\rho > 0$, one finds that if $\Delta > 0$, $\frac{\delta \mathcal{J}}{\delta x} > 0$ which seems to also yield an unstable situation.

Obviously in both cases we are not dealing with the correct proper perturbations corresponding to eigenvalues of the damping constant (or growth rate).

description of the physical process. Obviously, the stratified medium assumption is no longer fundamental in Cess's treatment.

THIRD EXAMPLE:

This last example is well known. It is simply the non-LTE radiative transfer problem, and the concept of thermalization length Λ , first introduced by Jefferies (1960).

In Mihalas's recent book (1970) the rather simple result obtained by Avrett and Hummer (1965), namely

$$\Lambda \approx \frac{1}{\epsilon} ; \frac{8}{9\epsilon} ; \frac{8a}{9\epsilon} \text{ (Doppler, Lorentz, Voigt),}$$

results from long calculations whose physical meaning is not obvious.

While the physical usefulness of Λ was demonstrated, for example by Rybicki (1971), for rapid calculations of non-LTE multilevel transfer problems, Athay and Skumanich (1971) succeeded in calculating orders of magnitude for Λ from very simple physical considerations. Notice again that the validity of this kind of procedure is demonstrated only because the "exact" solution is known! A series of papers by Finn and Jefferies (1968) and Finn (1971, 1972) also has to be mentioned; it deals with the probabilistic interpretation of radiative transfer. It is interesting to see the amount of formalism decrease while the physical insight given to the reader increases. The present tendency seems thus to eliminate most of the algebra, especially that connected with plane parallel geometry, and concentrates on the physical meaning of the local parameters. For example Athay (1972) proposes that the optical depth τ has to be replaced by the "mean number N of scatterings that a photon has to suffer before it escapes". Obviously there is a one to one correspondence between N and τ , but N is not related to a particular geometry.

In conclusion, I think that one can be optimistic about the possibilities that we now have to attack the problem of understanding the *local* machinery which makes the spicules, if we are careful to consider the right local parameters, and if we first try to get good local descriptions of physical processes.

INHOMOGENEITIES IN THE CHROMOSPHERE – CORONA TRANSITION REGION: MASS FLOW?

This paragraph is a brief account of a recent work (Delache, 1972) based on the two principles that have been stressed in the previous paragraphs:

- Try to define the local quantities which stand at the midpoint between observations and theoretical predictions. The proposal is to take the temperature as the independent variable (instead of altitude h , or optical depth), and to study the "thermal differential emission measure" $f(T)$ defined by

$$f(T) dT = n_e^2 dh.$$

- Relax the condition of a static atmosphere.

The equations are very similar to that of solar wind theory, except for radiative losses which are taken into account. In a first step, they are treated in a one dimensional analysis. The value of the velocity v , or mass flow $n_e v$, at a boundary will be physically fixed by the steady state, as usual, and will depend on the amount of energy deposited in the corona (I assume no energy deposition in the transition region). As this is outside the domain of the study, one will need the observations to infer v , either "local" observations (XUV spectrum or radiospectrum) or extrapolations of the solar wind flow.

First, one finds that $f(T)$ is, in fact, simply related to observations, either XUV or radio. If the pressure is assumed to be nearly constant in the transition region, then $f^{-1}(T) \propto T^2 \frac{dT}{dh}$; this last quantity is not very different from $T^{5/2} \frac{dT}{dh}$, which is the expression of the conductive flux. Thus, it is not surprising that simple reductions of observations lead so often to simple predictions of this flux. For example, Chiuderi et al. (1971) proposed a simple parametric representation of the radio observation. One can show that this particular form necessarily implies a constant conductive flux!

But the main result is the following: $f(T)$ can have two very different kinds of behaviour, depending on the value of the mass flow:

- If the mass flow is in the "low regime" (which would correspond to the solar wind flow, or less) then $f(T) \propto \sqrt{T}$, thus leading to a constant conductive flux and agreement with XUV observations for lines emitted at $T > 2 \cdot 10^5$ °K. This confirms Athay's previous result (1966), and is represented by the straight lines on Figure III-6 which is taken from Pottasch's classical work (1964). However, as can be seen on the figure, this behaviour does not match the observation for low values of T , nor does it match the radio observations (Lantos, 1971).
- If the mass flow is in the "high regime" (say 50 times higher than the prediction of a spherically symmetric extrapolation of the solar

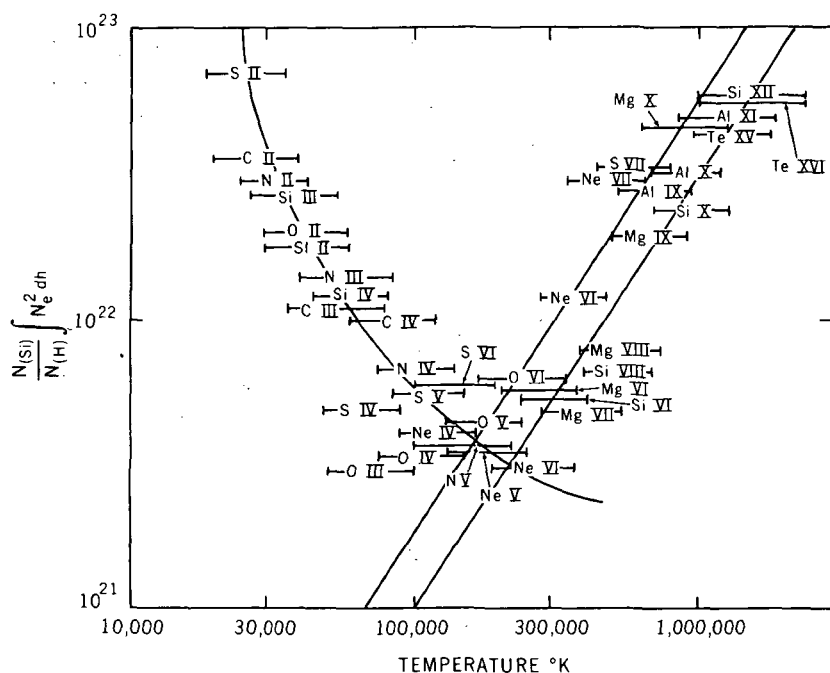


Figure III-6

wind), then $f(T) \propto T^{3/2} (T - T_0)^{-2}$ which agrees with radio observations and XUV observations for $T < 2 \cdot 10^5$ °K as shown on the left part of Figure III-6.

The two regimes can be reconciled in a single model in which the vertical coordinate is guided by the magnetic field. The cross section of the magnetic tubes of force open to the solar wind flow is increasing from the bottom (chromosphere) to the top by a factor of 50. Thus the mass flow $n_e v$ can be large locally, while it remains constant when integrated over the whole solar surface. This sort of morphology for the magnetic structures is known from observations of course, but it is striking that it can be deduced from observations which integrate the complete disk. The picture can be qualitatively completed: in regions of closed magnetic lines (i.e. the two ends are connected to the solar surface) the conduction perpendicular to the field is lowered; the outflow is prevented; the transition region should be very low in the atmosphere and very thin; it does not contribute to the emission measure for $T < 2 \cdot 10^5$ °K.

In this model, the transition region structure is dominated by the conductive flux for $T < 2 \cdot 10^5$ °K above spicules (open field regions) and for all T in the closed field regions. Below $T = 2 \cdot 10^5$ °K, in the open field regions, the enthalpy flux plays a major role. The motion of matter is important. (It has already been noticed by Kuperus and Athay (1967) that the energy flow due to motions in spicules was important.) The temperature gradient is not so steep. The amount of material in a given temperature range is increased.

CONCLUSION

It seems that we are now in a position of starting detailed physical studies of inhomogeneities. Local theories are being developed in dynamics as well as in radiative transfer. The mass flow has to be taken into account, as it is almost certainly a consequence of energy deposition. The momentum equation should also be looked at in detail, as the energy flow and deposition lead nearly always to momentum flow and deposition. (Pressure is exerted by the heating waves, especially in inhomogeneous structures, where they can be refracted.). The stability problem has to be solved after the non-static steady state is fully described. In the previous paragraph we have seen a crude theory starting on those basic principles, applied to a region where dynamics and radiative transfer are disentangled; one is really tempted to connect what is described there with spicular structure.

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DISCUSSION FOLLOWING THE INTRODUCTORY TALK BY DELACHE

Souffrin — I would like to ask where are the large and the small amplitude velocities that you talk about?

Delache — You may have "large" values for the boundary condition on the velocity, which means really a "large" value of the mass flux, if it would cover the whole Sun, while the numerical value for the actual velocity remains small. This is what happens in the lower part of the transition region.

Thomas — In other words, you end up with a small mass flux down below and a large mass flux up above.

Delache — The important condition is that the mass flux over the whole Sun remains constant. Also, I don't want to go into detail about the field structure. This whole picture I've given is very macroscopic.

Thomas — I haven't pushed you to open or closed fields. I've just pushed you to large or small mass fluxes, that's all.

Cayrel — Is it not true that if you multiply the mass of the spicules by the appropriate velocity you get the same order of magnitude as the mass flux of the solar wind?

Delache — Yes, I think Beckers has the answer, which I believe is yes.

Beckers — The upward transport in the spicules is two orders of magnitude greater than is required to balance the solar wind; but the energy available from the spicules is two orders of magnitude less than that required to balance the losses of the corona due to conduction down and other losses. So the spicules can easily provide the coronal mass losses, but not the coronal energy losses.

Underhill — I'm wondering if this has any relevance to a fairly commonly observed phenomenon in stars. In certain late type stars with extended atmospheres, you see what are called clouds. These clouds refer to the fact that one day you see two or three displaced calcium absorption lines and the next day you don't. This common type of observation can be explained qualitatively by irregularities in a more or less steady flow. I wonder if this solar-type flow you're describing here might be what is taking place? Could this kind of thing develop irregularities?

Delache — Yes, we know that this kind of thing can develop irregularities because the magnetic field structure is changing with time, often very rapidly. In the solar case, you must go to the filamentary structure. In observations of coronal streamers made from balloons, you see the structure changing in two or three hours.

Pecker — I'm a little troubled by the temperature picture that comes out of your model. The temperature within the spicules and the temperature outside the spicules seem to vary at such rates that it implies little connection between the thermal structure and the magnetic structure.

Delache — This is not really a complete model. For consistency, you have to demand something like pressure equilibrium between the two columns. That would require a further step than I have taken.

Skumanich — I think what all of you are saying is that some systematic flow is needed. On the other hand, this avoids the question that Thomas

raised long ago of the possible role of spicules in heating the corona. We tend to view the spicules now as though they arose from energy deposited in the corona and conducted back down into the chromosphere; but what about the possibility that they arise as a result of some hydromagnetic effect. We don't know what this effect may be, of course. We wave our hands and say Petcheck mechanism or induction mechanism, but the point is, couldn't some magnetic field effect be responsible for the spicules and might they not play some role in coronal heating? Do we have to go all the way down to the convection zone for the source of heat for the corona? Can we deduce anything from the ending of chromospheres along the main sequence? Can we say anything about how this convection decays as we go off the main sequence? Does the type of self excited instability that Ulrich has studied prevail along the main sequence? How does this scale?

Athay — One thing that excites me in your work is that you've taken data which have no spacial resolution and inferred an inhomogeneous structure for the Sun. This has important implications for stellar work. It's interesting that in the case of the Sun people working from a different direction arrive at the same results you discuss.

Pecker — I was intrigued by Anne Underhill's earlier comment. I wish she would make more clear to us exactly what stellar observations are relevant to these ideas of Delache.

Skumanich — I would like to know more about structure in extended atmospheres.

Underhill — The most pertinent observations are those given by Petore McKellar, and Wright on the 31 Cygni type stars. Regarding irregularities in the flow, you have variations in the tops of emission lines in the Wolf-Rayet type stars. Also in Be stars and B supergiants, when you can scan the profiles rapidly. You find they're changing in a matter of minutes, or at least a half hour. You just have to conclude from looking at the data that inhomogeneities exist.

Wright — The figure I discussed yesterday (Figure II-43) represents probably the best example we have of the satellite lines in the K line of 31 Cygni. This is a series taken during the eclipse of 1961, and I hope to observe a similar effect before May of this year. Here we have the normal K type spectrum with the emission produced by the K_1 and K_2 , and superimposed on that are the chromospheric lines as you come into totality. This series started in July of 1961, and by the time we got into August we saw evidence for these clouds or whatever you want to call them. This particular one lasted for three full days, August 6 to August 8. Then it disappeared and there was only a single component. Then in

September the additional chromospheric lines appeared again. Not in the same position, but since these are just velocity effects, this is probably due to the fact that different portions of the atmosphere are moving with different velocities at different times. Hence, the interpretation as prominences or clouds, whatever you want to call them. Sometimes you see several components. It doesn't show up too well here, but in 32 Cygni, particularly in 1965 and later on, I have suggested that they may be as many as four or five components at a single time. I'm not too positive about some of the multiple-component lines, but they do seem to be present. When you get deeper into the atmosphere, the lines tend to broaden out, and I am interpreting this broadening as the sum of several components. Finally, as you come out of totality, you begin to see the damping wings and get the true K line of calcium. But these must be velocity effects, I think. You have broad lines getting narrow and then broad again, all of which is evidence for the type of clouds that Anne Underhill was talking about.

Skumanich — Couldn't this be a binary effect, i.e., a gravitational perturbation, rather than a structural difference in the atmosphere?

Wright — I don't think so with these stars. you have the atmosphere extending out three stellar diameters, with the B type star just a little thing. There's no evidence I can find for mass exchange in 31 Cygni or Zeta Allrigal, for which we have this kind of data. There does seem to be mass exchange in UV Cephei, however, and it therefore qualifies as a close binary.

Pecker — I think those observations are exceedingly interesting, and I would be tempted to react in the same way Anne did. But this kind of thing could not be observed on the Sun at a distance, because the spicules are too small and numerous. So if you are to see the kind of thing discussed here, the elements must be of a sufficiently large size. So I ask the question to Philippe Delache of how one can apply the equations and conditions of energy, momentum, and mass conservation to these objects, keeping in mind the fact that much of the flow may occur out the side of the spicule-like inhomogenities.

Delache — I don't think you can do it, because the magnetic field is needed to confine the flow, and we don't know anything about the magnetic field structure of these stars.

Pecker — But the magnetic field *only confines* the flow. It does not alter the general picture regarding conservation of mass, momentum, and energy in the flow.

Thomas — The magnetic field only establishes the boundary physics — not the internal or overall physics.

Ulrich — I have a somewhat different point of view on this. I feel that the granule size or scale is governed by the pressure scale height somewhat below the surface of the Sun. Now the pressure scale height in these late type stars is a much larger fraction of the total radius of the star than in the case of the Sun. If you scale the granulation up proportional to the pressure scale height, you conclude that a granule in these stars is something like 8% of the radius of the star. Therefore the spicules are going to be very large objects. This does assume that the spicules are rather directly associated with the granules. Consequently, you don't necessarily have to have something like prominences to explain the observations; it could be something associated with the granulation. I would say that the case of 32 Cygni, the grazing eclipse, is an example of this.

Skumanich — A caution about scaling. As Durney has discovered in his work with Leibacher, one cannot always scale solar to stellar results, at least with the solar wind. They found that, in trying to scale the solar wind to late type supergiants, the sonic point was reached inside the radius of the star. So the wind there is more than the corresponding solar wind would be. It's a very dominant feature of the atmosphere. Although it's very useful to use the Sun as a standard, we should be particularly careful when we go off the main sequence. There are many changes to be taken into account.

Cayrel — In looking at the components of these K lines, I would like to ask what part comes from the main component of the atmosphere and what part comes from interspicular material?

Peterson — Isn't that really what Pasachoff has been observing? When we observe the K line with high resolution, aren't the changes due to the different chromospheric components we are observing?

Skumanich — I would be very cautious about that. I've looked at Pasachoff's results, and if we take, as a measure of the region we are looking at, the energy in the line over a one Angstrom band, we find that he was looking at only one network region. This problem of statistics does plague us, and we must be careful that we are looking at a representative solar region. Most of Pasachoff's data are from the cells. They are not from the network boundary. On the other hand, everyone has seen pictures of the Sun in $H\alpha$, .6 Angstroms from line center. Here we see little fingers which, if we identify them with spicules, show that they tend to cluster *around* the network boundary, presumably where the magnetic field is strong. Thus, you can see the K line from above without

having superimposed on it the time dependent spicule contribution. If not, we have a harder problem to solve. If we cannot assume a steady state, then we have to solve a dynamical transfer problem, and that is difficult.

Underhill — That's saying that you have radiative irregularities as well as spatial irregularities.

Skumanich — More than that. It's saying that while we've let the dynamicists worry about the time variable, we've ignored it in the transfer problem. I think that, in the network, that's all right. But in the spicules, that may not be all right. The spicules are a dynamic phenomenon, with time scales comparable to the reaction rates of interest.

Underhill — I can imagine a situation where you see the spicule for a while, and then you don't; so you think it has gone away. But maybe it hasn't, really. Rather, the spectral feature you were observing to detect the spicule has faded.

Skumanich — May I now call for more detailed questions on the two introductory papers.

Defouw — I'd like to make a comment on the first paper first. Jordan noted that shock waves may begin at an altitude of 1000 km. This is the altitude at which there is an abrupt temperature jump, and he implied that this temperature jump may be caused by this shock formation and the subsequent dissipation. I'd like to point out that the amount of dissipation that is required is not determined by the temperature but by the radiation rate. That is, an abrupt increase in temperature does not imply an abrupt increase in the heating rate. In fact, you can have an abrupt jump in temperature even if the heating rate is uniform throughout the whole atmosphere. To show this I will make an elementary calculation using the net heat-loss function, L , which is the cooling rate (in $\text{ergs cm}^{-3} \text{ sec}^{-1}$ or $\text{ergs gm}^{-1} \text{ sec}^{-1}$) minus the heating rate. The energy equation in which I am interested is $L = 0$. I would like to consider the simplest case where L depends only on the local values of the electron temperature, T , and the gas pressure, P . If we differentiate the heat equation $L(T, P) = 0$ with respect to height, h , . . .

Skumanich — Excuse me. Just for clarification, what is in your heating function L ?

Defouw — I'm going to consider the heating rate in L to be a constant. The function L includes the mechanical heating but is otherwise unspecified.

Skumanich — Do we know how to differentiate it?

Defouw — I'll differentiate it as follows:

$$\frac{\partial L}{\partial T} \frac{dT}{dh} + \frac{\partial L}{\partial P} \frac{dP}{dh} = 0.$$

Now I'm going to ignore momentum transfer by waves, which Delache likes to include. If we just consider ordinary hydrostatic equilibrium ($dP/dh = -\rho g$), we find that the temperature gradient is

$$\frac{dT}{dh} = \rho g \frac{\partial L / \partial P}{\partial L / \partial T}.$$

By the assumption $L = L(T, P)$, I've assumed an optically thin atmosphere. I'll draw on the board the radiation rate for an optically thin gas as a function of the electron temperature for a fixed value of density. This curve was first calculated in essence by Pottasch and most recently by Cox and Tucker. You have a maximum around 20,000 K due to hydrogen emission and a maximum around 100,000 K due to emission from ions of carbon and oxygen.

Skumanich — At what density?

Defouw — At any fixed density. If we consider fixed pressure, which is what we want for the derivative $\partial L / \partial T$, the cooling curve is similar but the maxima are shifted to slightly lower temperature.

Skumanich — Is there any particular density you would use?

Defouw — No, as long as the gas is optically thin.

Skumanich — I come back to my comment during the first day. That is not sufficient, there is a length scale that has to come into the problem.

Defouw — In this case, I'm just assuming an optically thin atmosphere. I don't believe the chromosphere is really optically thin. This is just an illustrative calculation. Now, the numerator ($\partial L / \partial P$) of the above expression for dT/dh is always positive because it is essentially a density derivative of the radiation rate. The sign of the denominator ($\partial L / \partial T$) is determined by which side of a maximum in the cooling curve you are on. If you are on the low-temperature side of one of the maxima, the denominator is positive, and the temperature must increase with height in order to keep the radiation rate equal to the heating rate. As you get closer to the maximum, the radiation rate becomes less sensitive to the temperature, and therefore the temperature has to increase more rapidly with height. Finally, at the maximum, $\partial L / \partial T$ vanishes and the temperature gradient becomes infinite. By this time conduction has become important.

If you look at models such as the one Vernazza presented the other day, and early models of Thomas and Athay, you see two temperature jumps which I think you can associate with the two maxima in the cooling curve. I admit that optical thinness is not a valid assumption near $T = 10^4$ °K, but I think it is reasonable to assume that the temperature dependence of radiation will show a maximum due to hydrogen emission. The first temperature jump near $T = 10^4$ °K, should be attributed to this hydrogen maximum while the chromosphere-corona transition is due to the carbon-oxygen maximum in the cooling curve. The largest temperature gradient occurs where the maximum in the radiation rate is found. It follows that we are not jumping to coronal temperatures because we need more efficient radiation—we are already at the maximum of radiation efficiency. Because we are at the maximum, the denominator in the above expression for dT/dh vanishes, and we have an infinite temperature gradient. That is my first point.

Now I'd like to comment on the model of Delache. This comment may be wrong because I'm not sure I understand the model. If so, please correct me. We have the large conductive flux from the corona. How do you dispose of this flux? Radiation cannot dispose of it because the temperature gradient near $T = 10^5$ K is so large that the large conductive flux from the corona is deposited in a shell only a few kilometers thick. This problem was first pointed out by Giovanelli in 1949. Now, what Delache proposes to do is to balance this conductive flux with an enthalpy flux associated with some fluid flow. He finds first, doing a one-dimensional calculation with no horizontal structure, that, if the enthalpy flux is to be large enough, you need a fluid velocity of 50 km/sec, or several tens of km/sec. The mass flux you get for these velocities is much larger than the mass flux in the solar wind. To get around this, he says that the velocities are occurring just over a fraction of the disc, to reduce the mass flux. So he still has velocities of 50 km/sec.

Skumanich — I think 10 km/sec was the value.

Defouw — OK. 10 km/sec. As I understand it, he has not done the energy calculation for this new configuration. One thing he is obviously going to have to include is what Kopp and Kuperus pointed out. The conductive flux is also going to be channeled by the magnetic field and it's going to be magnified by the same factor that the mass flux is reduced. So it is not at all clear to me that the enthalpy flux associated with the mass flow will still balance the conductive flux.

Delache — I think that the answer is yes, that the kinetic flux is channeled by the magnetic field, but you must realize that you do not necessarily have conservation of the whole conductive flux, as is the case for the mass flux.

Defouw — Then is it true that you are no longer balancing the enthalpy flux with the conductive flux?

Delache — If we include radiative losses, that is true. As I said, you are increasing the gas conductivity and lowering the temperature gradient, which increases the omission measure and the amount of material which can emit radiation.

Defouw — Now do you think the radiation can take over?

Delache — No, it can only partially take over.

Defouw — Have you done the complete calculation for the configuration?

Delache — As I have said, this model is very naive, with one thing on top of another. We have to go through this region with a variable cross section, which I have not done yet. But in this discontinuous model, the basic quantities are conserved: mass flux, and energy (flow, conduction, radiation).

Defouw — My last comment is that I no longer believe in my theory of spicules. The reason I don't is that the temperature of spicules seems to be going down. The most recent estimates are about 8000°K . For thermal-convective instability you need at least $12,000^{\circ}\text{K}$.

Skumanich — They are 8000°K if steady state is assumed. So we are hiding a sinner in the basket, for, if steady state does not hold in spicules, the estimate of the temperature may be in error. I don't know by how much, but I don't feel that your suggestion is necessarily thrown out by current low temperature values based on a steady state assumption.

Defouw — I believe that my explanation of the temperature jumps is essentially correct, although some details like opacity effects and the height dependence of the true heating rate will require some modifications.

Ulmschneider — It seems to me that observations show radiation losses in Lyman α , and so on, that are much greater than the C, N, and O radiation loss.

Defouw — But the observed line intensities depend on the temperature gradients in the respective regions of line formation.

Ulmschneider — This curve that you plot here should be such that the H peak should be very large and the C, N, and the O peaks quite small, on the tail of the H. (Editor's note: This curve does not appear in these Proceedings.)

Defouw – You can't proceed from observations on this matter, because the observed intensity of a line depends on the thickness of the region of line formation.

Skumanich – Well, that's not quite correct, because I think we do have to accept the spectroscopic model of Mr. Vernazza.

Defouw – I think that in Vernazza's model, Lyman α is produced in a region 100-200 km thick, whereas the important carbon and oxygen lines are formed around $T = 10^5$ K, where the length scale is only about 5 km.

Schwartz – Let me make a comment on what Defouw just described in a qualitative way for a constant heating rate, and say that it probably occurs even in a more realistic situation, Figure III-7 shows the results of a calculation showing, in a quasi-realistic way, the heating and cooling of this region. It is a numerical experiment, where you take the atmosphere

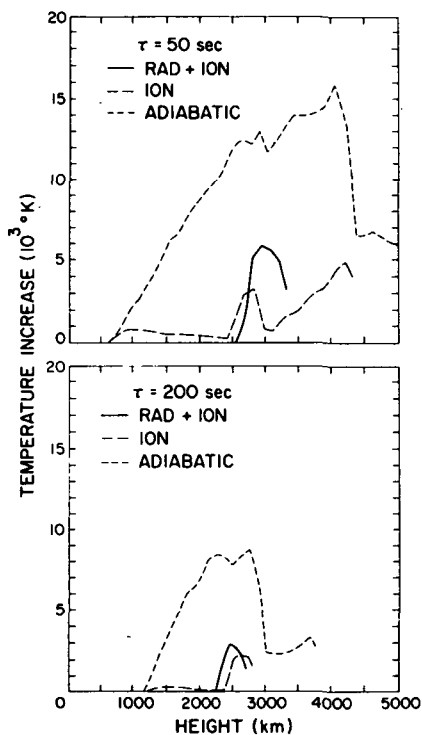


Figure III-7

and tickle it from below, and then watch and see what happens. The upper curve is for a half-sine pulse with a width of 50 seconds (for a full sine wave it corresponds to a 100 sec period). The first pass at the problem didn't include any radiation at all to cool the atmosphere. We just set the radiative cooling equal to zero, and the temperature just went shooting up as soon as the shock was formed. When we put in the sort of cooling that Defouw talked about, the temperature rise was rather modest until you got to something like the transition region; then the temperature shot up again. However, the net dissipation of mechanical energy as a function of height was nearly the same for these two calculations. You see that the inclusion of radiation causes a rather radical change in the temperature distribution.

Skumanich — It sounds as though the mechanical dissipation is temperature insensitive.

Schwartz — The dissipation was fairly temperature insensitive, but the temperature rise produced by that dissipation is affected very much by the radiation.

Stein — I would like to make three comments about shocks, before Bob Schwartz continues with the results of our computer experiments. First, an isolated pulse and a train of waves behave very differently. For an isolated pulse the shock strength increases indefinitely as the wave propagates outward. In an isothermal atmosphere,

$$(M - 1) = (M - 1)_0 \frac{e^{z/2H}}{\left[4 \frac{H}{\lambda_0} (M - 1)_0 (e^{z/2H} - 1) + 1 \right]^{1/2}},$$

where M is the Mach number and the subscript 0 indicates initial value. When the atmosphere has a more complicated structure, corresponding formulas can be obtained, but I just want to show the simplest case. For a train of waves, however, the shock strength, instead of increasing with height, approaches a constant asymptotic value;

$$M - 1 = (M - 1)_0 \frac{e^{z/2H}}{4 \frac{H}{\lambda_0} (M - 1)_0 (e^{z/2H} - 1) + 1}.$$

So the first thing you should decide when making a model is which is the realistic situation for the Sun.

Second, weak shock theory is an infinite frequency theory. It includes stratification, but neglects the dynamic effects of gravity. This effect is dramatically illustrated by some of our results which Bob will present.

Finally, even for high frequency waves, weak shock theory gives incorrect dissipation. A wave must propagate some distance before it forms a shock and begins to dissipate, so weak shock theory which assumes that a shock already exists cannot be applied starting at the place where the wave is produced. On the other hand, after a shock has travelled a distance, non-linear effects increase the dissipation above the weak shock value.

Skumanich — Does what you say depend on the temperature structure of the atmosphere?

Stein — Not really. Weak shock theory with these simple formulas is for an isothermal atmosphere. The same thing happens if the atmosphere has some nonisothermal temperature structure, but then the formulas come out in terms of an integral over height. Qualitatively, the behavior is the same.

Ulmschneider — I would like to make one comment. Until now, the weak shock theory was only applied a considerable distance above temperature minimum to insure that the shock would be fully developed. Therefore the dissipation is naturally too large at low heights, if weak shock theory is erroneously used there.

Skumanich — So what you are saying is that you can "fudge" where you put the energy by where you introduce the shock. Is that correct?

Ulmschneider — Yes, to date, when we used a fully developed weak shock theory, we didn't start from the temperature minimum, but started from an observational point further up.

Skumanich — How can you justify that? What is the reason for putting the boundary where it is?

Ulmschneider — Because I know that the shock is not developed lower down and that you cannot expect the result of a fully developed weak shock to be correct there.

Jordan — I agree with Ulmschneider that one can invoke fully developed, weak shock theory in the manner he indicated and still obtain reasonable dissipation estimates above the temperature minimum. This is because, from independent studies, it is just above the minimum that we expect significant departures from radiative equilibrium, largely due to H^+ , and also because the shock strength settles down to a value of about $1/3$ over most of the low chromosphere, rather independent of the initial value η_0 for the strength chosen (in a reasonable range of $0.1 < \eta_0 < 1/3$). Thus, any overestimate would be confined to a narrow region just above the minimum. Furthermore, one can follow the development of an initially sinusoidal sound wave from the low photosphere, where it is generated, to

the chromosphere. Many independent studies suggest that these waves will become fully developed shocks (for short period waves with period $T < 100$ sec) by the time they have reached the low chromosphere above the minimum.

Skumanich — It seems to me that the correct solution is to get Vernazza's model. He gives you the mechanical flux gradient without the conduction term taken into consideration; but he presumably can put that in. The question is, are you getting that kind of dissipation of energy with height, or with density, as Vernazza suggests, or not? It is not clear that you are. You seem to put the lower boundary wherever it suits your purpose.

Jordan — But if the computed net radiative losses as a function of height correspond to the computed mechanical dissipation as a function of height for the same model, as the calculations we have done with the HSRA so far indicate, then this is reason to believe that the initial formulation chosen for the problem is not too unreasonable. This is the criterion you have just stated.

Schwartz — Let me tell you where to put the boundary. If you start at height zero with a wave of initial velocity v_0 and period equal to τ , then the wave goes a distance $\Delta Z = 2H \ln(1 + \gamma/2(\gamma + 1) \cdot g\tau/v_0)$ before the crest of the wave has caught up with the trough and it has formed (in some sense) a fully developed shock. If you start off the wave with a given velocity amplitude, then this formula tells you at what height you can begin to apply these weak shock formulas.

Souffrin — Is that the distance where you start pushing the gas?

Schwartz — That is where the shock is formed.

Souffrin — You put in v_0 and you get the shock at some distance?

Schwartz — That's correct.

Souffrin — In the distance travelled in a period or two, you reach the place where the shock forms?

Schwartz — That's correct. This says if you take longer periods the wave gets higher up before it shocks. But remember this is an oversimplification which neglects gravity.

Skumanich — What are the free parameters in this? It looks like they are v_0 and the height at which you hit the atmosphere with v_0 with an infinite plane wave.

Schwartz — This is for an isothermal atmosphere which is the only condition in which you can work it out analytically.

Skumanich — But what are the parameters? v_0 , the height at which you start the pulse, and what else?

Stein — In the Sun, essentially nothing. Even the height at which you start is rather unimportant because, once you get into the convection zone, the density scale height becomes large and, therefore, starting the wave deeper in the convection zone will not change very much the height at which the shock forms.

Schwartz — In other words, this formula fails, because the atmosphere is not isothermal below the top of the convection zone.

A. Wilson — If I give you some numbers, I wonder if you can tell us what that Δz would be under those circumstances. Try a period of 10-20 sec, and an amplitude of 1.2 km/sec. What is Δz ?

Stein — We didn't actually do it for a 20 sec, but for a 50 sec, pulse in our paper and it came out to be $3\frac{1}{2}$ scale heights. This is the point at which you start getting dissipation.

Skumanich — Where does that put you relative to $h = 0$ on the limb?

Stein — A few hundred kilometers higher.

Skumanich — But isn't that too low?

Stein — No, not for a high frequency, like the 50 sec pulse represents. Longer period waves, on the other hand, don't form shocks until they reach greater heights.

Ulrich — I want to make a general comment about all of this sound wave heating. I think unless you put some treatment of the radiative interaction in the dynamics of the sound wave you are not likely to get the correct answer. This is a very dominant effect. It makes the calculations messy. I don't know how the energetics work out. I would be surprised if you got the same results.

Stein — We also did the calculation with radiation. We included H^- and hydrogen recombination to excited states in an optically thin approximation. Direct radiative damping of the waves occurs in the photosphere and low chromosphere, and can remove up to $2/3$ of the wave's energy. The rate of shock dissipation is insensitive to radiation, but instead of the temperature increasing this energy goes into ionization and radiation. The temperature rise is small until hydrogen is ionized.

Souffrin — Regarding what you just said, and what Ulrich just said before, about pulses and wave trains. It is somewhat like the difference between an initial value analysis and a boundary value analysis. Consider the question of dissipation. It turns out that it is just impossible to guess

ahead what effect radiation will have on either analysis. If you look at the initial value problem, you have some motion given at an initial time. Then suppose you have some radiative damping. It turns out that radiation smooths out the motion in a given time. If you look at the boundary value analysis, you just can't guess, due to stratification, anything about the spatial damping in the wave train problem just by looking at the time damping in the initial value problem. It is very important in all these questions concerning heating to have a good idea about the physics of the excitation of the observed motion.

Schwartz — For the weak shock theory, which Jordan just talked about, we did another numerical experiment. This time we excited the atmosphere with a wave at the bottom with a period of 100 sec. and let it run for about 40 periods to let the transients die down and enable the atmosphere to achieve something like a steady state. The velocity profile is shown on Figure III-8. This resembles a classic N wave, as everybody has assumed in treating this problem. However, you will notice that the

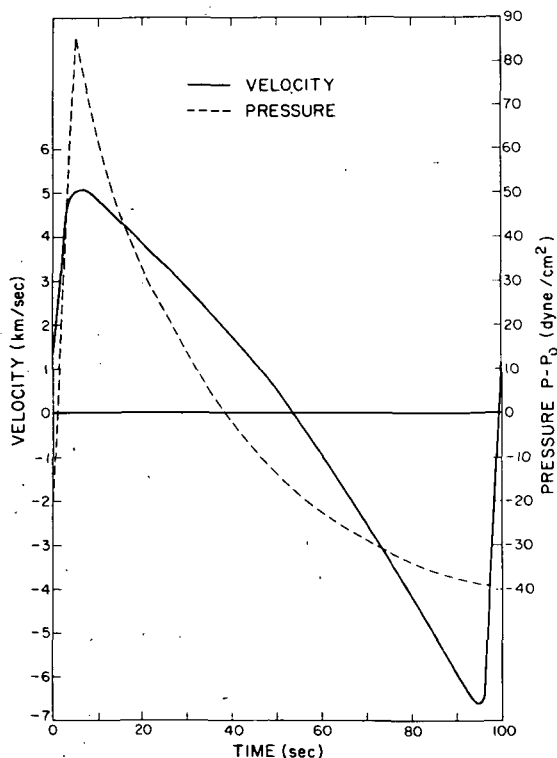


Figure III-8

pressure variation is much more sharply peaked than the velocity variation. That might be of interest for people who look for oscillations in intensity. Figure III-9 shows the same calculation for a wave with a period of 400 sec., about half the acoustic cutoff frequency, twice the critical period. This is in the non-propagating region in the low chromosphere. Here, at 1000 km above the photosphere, it still looks very sinusoidal, and the pressure variation is very smooth. You will notice that, in this wave, the pressure is out of phase with the velocity. Look at the relationship Jordan wrote down this morning for the energy propagation: energy flux =

$$F_m = \frac{1}{T} \int_0^T (P - P_0) V dt .$$

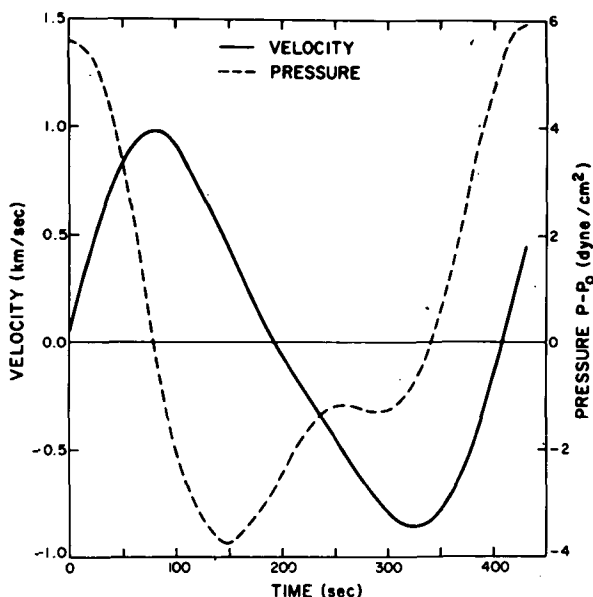


Figure III-9

Since the pressure and velocity are 90° out of phase, then the wave is not propagating energy; it is like a standing wave.

Figure III-10 shows the dissipation caused by the first wave (100 sec period). The dotted line is the fully developed weak shock theory and the solid curve is the result of the actual non-linear numerical calculation. Although it has the same asymptotic form, it still disagrees by an order of magnitude in the asymptotic regime, even though this is the regime where you might expect the weak shock theory to hold. Of course, as

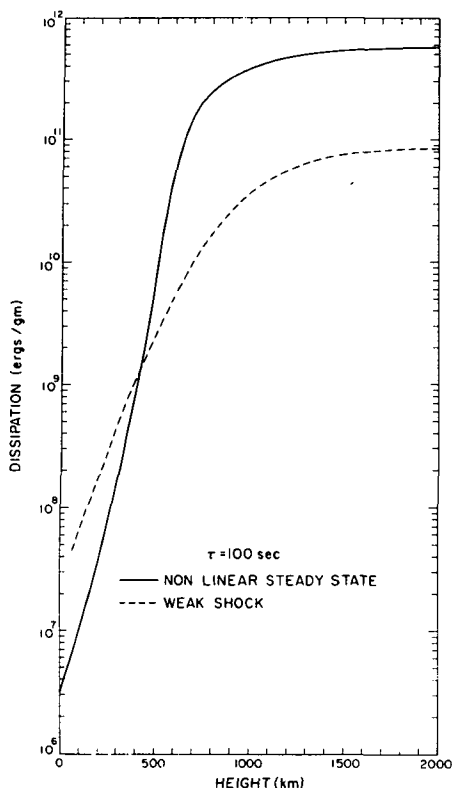


Figure III-10

Ulmschneider just remarked, the weak shock theory gives excess dissipation in the lower atmosphere, because it assumes a fully developed shock at all heights.

Ulmschneider — Nobody has done a calculation down there using the weak shock theory.

Schwartz — That's correct, for the reason you pointed out. You know the weak shock theory will give the wrong result down there.

Skumanich — What, exactly, are you comparing to what?

Schwartz — I am comparing the weak shock theory to the exact integration of the equations of motion, for this particular model, the isothermal atmosphere. Whether or not it has anything to do with the Sun or not is another question. However, in this example, the heating which is given by the weak shock theory above 1200 km. is an order of magnitude less than the exact solution which the weak shock theory

purports to approximate. If you think this is bad, look at Figure III-11. That gives results for a long period wave, the 400 sec one. We see the

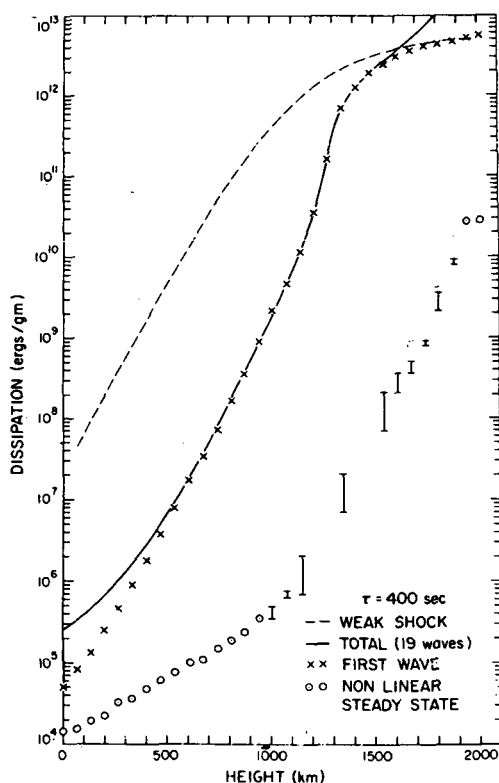


Figure III-11

weak shock result for the dissipation here, and the steady state result for the non-linear calculation down below by 5-6 powers of ten. So the application of the weak shock theory to waves with periods longer than the acoustic cutoff is nonsense, even qualitatively.

Ulrich — May I please ask whether your exact equations of motion included the radiation curve?

Stein — In this particular case, to make comparisons easier, the comparisons were between the weak shock and an exact integration of the equations of motion for an isothermal wave in an isothermal atmosphere.

Schwartz — We have obtained results which include radiation, in the optically thin approximation.

Souffrin — For an isothermal shock in an isothermal atmosphere there is an isothermal deformation, and the dissipation goes to zero.

Schwartz — If you have a shock you have dissipation.

Souffrin — For an isothermal wave, its dissipation goes to zero. Gamma is one.

Schwartz — No, it is $\int Tds$ and the entropy changes across the shock. But you assume, by saying it is isothermal, that this is a very unphysical radiation shock. As soon as you raise the temperature slightly above the ambient temperature, the system gets rid of all the energy by radiation as fast as you dump it in. That is physically what this means. But it's not a physical result.

Skumanich — What you are saying is that you are throwing away the entropy generated by the shocks, so it doesn't go into internal energy.

Schwartz — That's right, but you are keeping track of the total amount you have thrown away; that is what this dissipation is. This is admittedly an unrealistic case, but it was the only case for which we could write down an analytic formula for the solution of the weak shock equations to compare it to the numerical results.

Skumanich — I infer that you are then saying, "Don't trust weak shock theory." Now, do the weak shock theorists want to stand up and say something in rebuttal.

Ulmschneider — This work was done with waves of higher period. The weak shock theory is mostly done with waves of about 25 sec period. This is a factor of four below the waves discussed here. So I would suggest that by extrapolating in Schwartz's graphs from the 400 sec to the 100 sec and from there to the 25 sec waves that the weak shock result would be much better for the higher frequency waves.

Jordan — To that, I would like to add something that has already been pointed out; namely, in the application of this theory, we do not have to assume a fully developed shock at the zero point on those graphs. So I would completely agree with everything on Schwartz's graphs, and, yet, I think that the weak shock theory does have a useful range of validity for high frequency waves in the low chromosphere.

Skumanich — Then the question is; are they high frequency or low frequency waves?

Stein — In the discussion this morning, Beckers talked about the observations and said that they had looked at phenomena with good enough time resolution so that if there had been shock waves with periods of about 50 sec they should have seen them, and they didn't.

Thomas — Wait a minute. You have given a shock wave and you should predict what you should expect to see. Then you ask whether Beckers has seen it or not.

Stein — Wolfgang, Kalkofen and I are in the process of doing that.

Schwartz — It should be noted that it is a bit misleading to talk about high and low frequencies in this context. Although a wave may be thought of as starting out with a certain frequency or superposition of frequencies, the situation is altered once the shock forms. Since the shock travels faster than the sound speed, it catches up whatever waves may be present ahead of it and converts some of their energy into shock energy. Thus, although you may have, for instance, some transient low-frequency waves present in the atmosphere which would never form shocks by themselves, that does not preclude their contributing to the strength of a shock by this nonlinear interaction.

Ulmschneider — By considering what kind of mechanical flux you have, you find that the amplitude of the shock wave is not very large. You don't expect a very high velocity of the shock. You expect a shock velocity that is almost equal to the sound velocity. In that way the shock doesn't eat up the other disturbances. It appears that the high frequency waves which have periods of around 10-20 sec develop into shocks first at low heights. You can get an idea of how such a shock develops out of a sound wave by considering the simple wave theory. This shows that if you assume the same initial flux, then a wave of high frequency will develop into a shock earlier. This was the basis for my work, in which I assume that at low heights, 200 km above the temperature minimum, high frequency shock waves are formed. In the case of short period waves, as we just saw, I suppose it isn't too bad to assume weak shock theory. I think everything is self consistent and consistent with the computation of acoustic flux done by Stein. The graphs shown by Jordan show that a large part of the acoustic flux comes in short period waves. Now, why don't you see them? I think, because of the contribution function, the height interval in which you contribute to the line emission is about 300 km. But if you have a 10 sec period wave and a 7 km/sec sound speed, then you have a wavelength of 70 km. This fits several times into 300 km, so you shouldn't see it. You shouldn't see effects of high frequency sound waves in spectral lines. It will add to the microturbulence of the medium, so you see a broadened profile. But will not see a periodic shift of any kind. So I don't think this is an argument against high frequency waves.

Thomas — But how much can it add to the microturbulence? You had a very high frequency weak shock, so how much does the material velocity

change across that shock? If it's not much, then it doesn't do anything to the microturbulence.

Ulrich — There must be a sound speed difference across the front or it's not a shock.

Thomas — An infinitesimally weak shock has no material velocity amplitude, and that's what I measure. The propagation velocity is the sound velocity, but not the material velocity change.

Souffrin — There is a difference between turbulent excitation and pulse excitation. If you think of just one shock traveling, then, after some time, the initial situation is restored. Now from a stationary excitation there must be a stationary structure, something that is not a traveling shock.

Skumanich — One comment. We have been talking about two modes of excitation. One is from the turbulent convective region, and it propagates and then undergoes non-linear interaction. The second one is just like a piston hitting bottom of the atmosphere with some characteristic time. Which one is involved in producing these high frequency waves? Also, will the mechanical energy theorists please give the observationalists some guide as to what they should be observing. The theorists have to constrain the domain of applicability so that some observational parameters can be found.

Ulrich — There are four main points I wish to make. I will start off by some discussion of shock waves, because I think the evidence in favor of them is weak from the observational standpoint. At least for the longer period oscillations, the observed line core intensity fluctuations are too small to be compatible with shocks. If there are shocks, they must occur at higher frequency. I think this particular effect was demonstrated by the observations of Simon and Shimabakuro (Ap. J., 168, 525), who looked at the electron temperature of the gas somewhere about 2000 km above the temperature minimum. They found that the brightness of this gas showed only a slight correlation with five minutes. Their time resolution probably was not able to provide useful results for periods shorter than 100 sec.

Concerning the power spectra calculated by Stein, I would point out that, in the region of the frequency diagram between 100 and 300 sec, it gives exactly the wrong slope compared to the observed power. Anyone using this theory should find a reason for this error and come up with another power spectrum which is more in agreement with the observations. Until this is done, I, for one, will remain a little skeptical of this peak at 50 sec. That is something that the high frequency shock people must do in order to make their theory more believable to me.

In talking about heating today, we have had a good deal of discussion about the derivative of the mechanical flux. Figure III-12 illustrates some of the relevant expressions. Most people have used the first expression for

| | |
|---|--|
| CHROMOSPHERE HEATING WITHOUT SHOCK WAVES | |
| F = FLUX OF ENERGY ASSOCIATED WITH OSCILLATORY MOTIONS | |
| $16 \sigma T^3 \rho \eta (T - T_{eq}) = - \frac{dF}{dz}$ | : ACOUSTIC ENERGY BECOMES RADIATIVE ENERGY |
| WHAT IS F? THE CIRCLED QUANTITIES ARE CLAIMED TO BE F. | |
| ECKART | : $\rho \frac{D\mathcal{E}}{Dt} + \nabla \cdot (\rho \underline{u}) = \rho \underline{g} \cdot \underline{u} + \rho q$ |
| LANDAU & LIFSHITZ | : $\frac{\partial}{\partial t} (\rho \mathcal{E}) + \nabla \cdot (\rho H \underline{u}) = \rho q$ |
| WHERE | $\mathcal{E} = \frac{1}{2} \underline{u} ^2 + E + gz$ |
| | $q = - 16 \sigma T^3 \eta (T - T_{eq})$ |

Figure III-12

the mechanical flux. I have tried to find the source of this expression, and the references lead to the book by Eckart on "Hydrodynamics of Oceans and Atmospheres" in which this equation is indicated. This is the total derivative of the energy per gram of the fluid following the motion. This equation seems perfectly valid. However, I think it's hardly clear that the circled quantity is a proper flux since the divergence of this must follow the motion of the fluid. Additionally, there are two extra terms. Landau and Lifshitz, on the other hand, derive this equation, and point to this term where the script H is the enthalpy and claim that this is the mechanical flux. On the left hand side is a time derivative fixed in space so that Landau and Lifshitz's flux looks like a more legitimate flux since the divergence of it gives a time derivative of the energy density. I have adopted this as my definition of the mechanical flux. In the case of non-adiabatic oscillations this expression gives an additional entropy derivative which must be included in the flux. Another thing to notice in this equation is that I have written the emissivity schematically in a crude form. It is precisely this same quantity divided by the density which appears in the equations of motion of the sound wave.

I have studied the propagation of acoustic waves in the presence of a temperature gradient and radiative interactions. Figure III-13 shows the assumptions that I have used to get a tractable dispersion relationship. The critical assumption here is that the opacity is given by LTE.

ASSUMPTIONS

1. SMALL AMPLITUDE OSCILLATIONS ABOUT A PLANE PARALLEL MODEL IN HYDROSTATIC EQUILIBRIUM.
2. PERFECT GAS EQUATION OF STATE.
3. OPACITY GIVEN BY INSTANTANEOUS LTE.
4. NO VISCOSITY.
5. $\eta T^3 = \text{CONSTANT}$.

Figure III-13

Relaxation of that assumption would give a different radiative cooling rate and possibly a phase shift. This assumption of LTE says that the radiative cooling goes towards wiping out a temperature difference between the average medium and the displaced parcel.

Now for the second point. I want to demonstrate that overstability is possible whenever the temperature gradient exceeds a critical value which is less than the adiabatic gradient. As a way of convincing you that this is at least possible, I would like to present the following rough argument. Consider an atmosphere initially in hydrostatic equilibrium. Label mass shells in this atmosphere by their undisplaced altitudes z and consider plane parallel displacements $\xi(z)$ about these altitudes. The continuity equation then gives

$$\frac{\Delta \rho}{\rho} = -\frac{\partial \xi}{\partial z},$$

and the momentum equation gives

$$\frac{\partial^2 \xi}{\partial t^2} = \frac{\partial \Delta P}{\partial z},$$

The quantities ΔP and $\Delta \rho$ are the changes in pressure and density following the motion. In the case of an isothermal atmosphere and adiabatic displacement, the solution to these equations is well known and is (see Lamb, 1940, "Hydrodynamics," § 309)

$$\frac{\partial (\rho^{1/2} \xi)}{\partial z} = \left(\frac{\omega^2 - \omega_0^2}{c^2} \right)^{1/2} \rho^{1/2} \xi$$

where ω is the frequency of the wave, ω_0 is the acoustic cutoff frequency and c is the adiabatic sound velocity. At $\omega = \omega_0$ we see that $\partial(\rho^{1/2} \xi)/\partial z = 0$. Therefore $\partial \xi / \partial z = \xi / (2H)$, where H is the pressure scale height. Using the continuity equation we conclude that

$$\frac{\Delta \rho}{\rho} = -\frac{\xi}{2H}.$$

This implies an adiabatic temperature change of

$$\frac{\Delta T}{T} = -(\gamma - 1) \frac{\Delta \rho}{\rho} = (\gamma - 1) \frac{\xi}{2H}.$$

The condition for overstability in the case of slow radiative heat exchange requires that the rate of change of the temperature in the blob $|YT/\xi|$ exceeds the rate of change of temperature in the surroundings $|dT/dz|$. In terms of the logarithmic temperature gradient this condition is

$$\nabla > \frac{\gamma - 1}{2},$$

which is always weaker than

$$\nabla > \frac{\gamma - 1}{\gamma}.$$

This condition differs from the usual condition for the onset of convection because the pressure in the displaced blob of matter is not equal to the average pressure. I find this same condition from the correct

solution to the equations of motion with a temperature gradient in the case of slow radiative heat exchange. In the case of a very rapid radiative heat exchange I find the condition is

$$\nabla = \nabla_{\text{ad}}/2$$

but at present I do not have a short derivation of this condition.

The third point I want to make concerns the temperature rise. After computing the divergence of the flux associated with an acoustic wave, you can determine the temperature rise required to dispose of this energy. The equation I find is

$$T - T_{(\text{rad. equi.})} = 350^\circ\text{K} \left\{ \left(v_{\text{rms}}^2 \right) \cdot K \left(\frac{\omega}{\omega_0}, \frac{\beta}{\omega_0} \right) \cdot \frac{2.5R}{C_p \mu} \right\},$$

where v_{rms} is the material velocity in km sec^{-1} , K is a function which varies in value according to the graphs of Figures III-14 and III-15 as a function of radiative damping parameter β and the ratio ω/ω_0 , C_p is the specific heat at constant pressure, R is the gas constant, and μ is the mean molecular weight. The largest values of K occur for the lowest frequency and ω and the smallest values of β . At small β and low frequency you get a fairly large factor. If you put in an rms velocity like 4 km/sec, and if this value occurs at low frequency, then you get a very large temperature rise from this formula.

Another thing to note is that, for small β , $T - T_{(\text{rad. equi.})}$ is independent of β . At small β , if the medium can dispose of the energy quickly, then it gets a large share of the acoustic flux which comes by. On the other hand, a section of matter which cannot radiate easily does not get a very large share of the passing acoustic flux. This type of heating seems to be a rather democratic process where those who pay (radiate) receive a large share of the money (energy) and vice versa.

A final point which concerns the five minute oscillations is something of a puzzle. If you believe that the 5 min. oscillations are heating the chromosphere, then you have the disconcerting observation that the amplitude of the five min. oscillations is less under plage regions than under quiet regions. This means that you are generating less energy, since the energy generation in overstable acoustic waves is proportional to the square of the amplitude. Yet, there seems to be more emission in the higher layers. This is a puzzle. I think one possible explanation is that in a magnetic region, the required emission is redistributed and it is easier for an upper layer to radiate the energy which is being generated. So you need a smaller amplitude to drive the whole thing. This explanation does

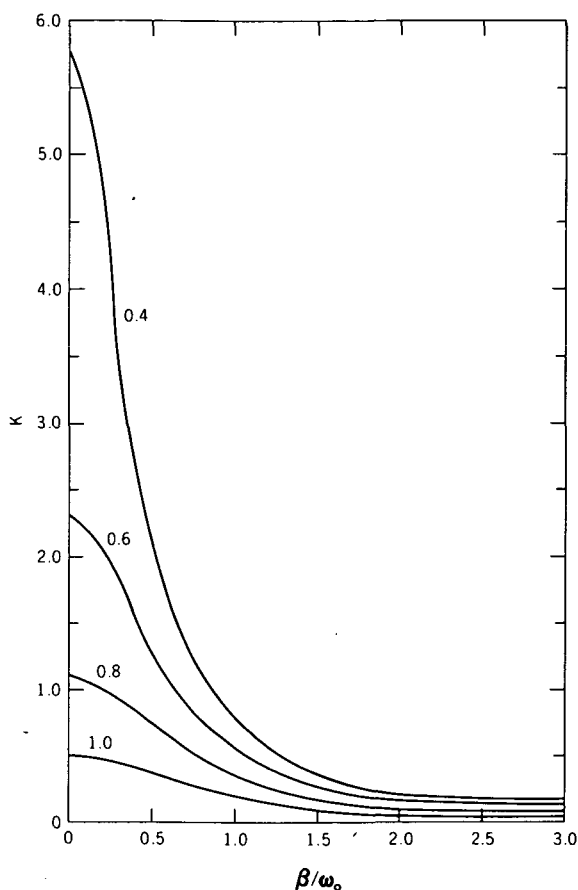


Figure III-14

not seem very satisfactory, however, and, yet, I don't have a better one. Possibly there is another energy source over plage regions which is dominant.

Skumanich — What is the reservoir from which the work comes, is it the radiation field?

Ulrich — It's the oscillations in the convection zone. Ultimately, that is the source. The energy emitted locally in the low chromosphere and observed as a temperature excess has as its immediate source the pressure variations of the underlying layers. These in turn are generated by the interaction of the radiation exchange and the temperature gradient. The temperature gradient permits a displaced parcel of fluid to be cooler than average when it is in the compressed portion of the oscillation cycle. As

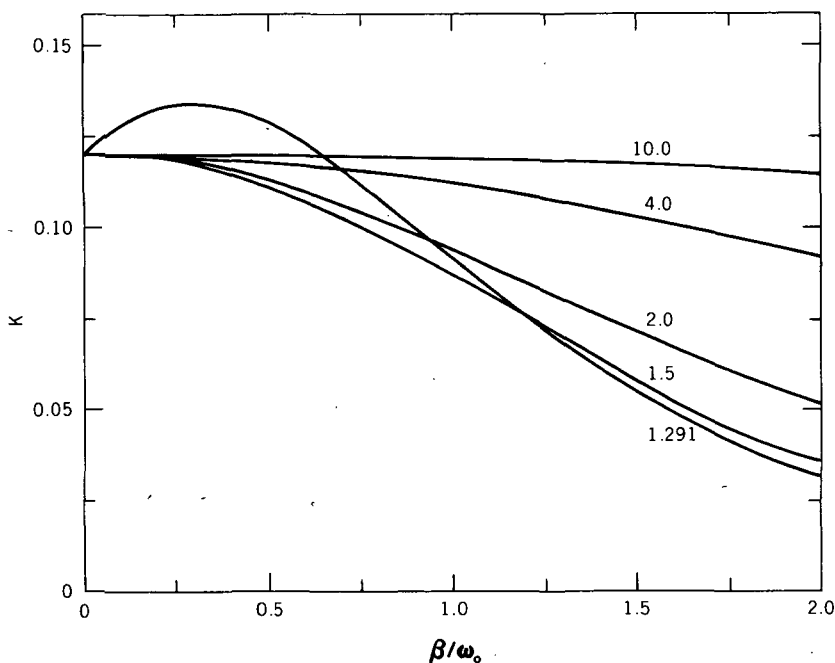


Figure III-15

long as the matter gains thermal energy at this phase, then the oscillations will be driven as in a classical heat engine. As far as the immediate source of energy for the excess radiation is concerned, it doesn't matter what drives the acoustic waves. Also I should say that, at this point, I haven't said anything about the overall energy balance of the oscillations. This must be considered to determine the amplitude.

Thomas — I don't understand that remark.

Ulrich — In terms of the 5 min. oscillations, I think the analysis must be essentially non-linear, such as that done by Leibacher.

Thomas — What is your basic coupling mechanism between the aerodynamics of the motion and the radiative energy balance in the electron continuum?

Ulrich — This is just the work which is done over a cycle by the compression.

Thomas — So if I have a big radiative energy loss, I can hold this amplitude down.

Ulrich — If you have a big radiative energy loss, then, for the same amplitude of oscillation, you get a larger portion of the work out of it.

Thomas — I am talking about the temperature amplitude now. If I have a big radiative energy loss, I can hold this amplitude way down. I can't see from your equations where you have these things put in. There must be a coupling term somehow.

Ulrich — Perhaps I can clarify a crucial aspect of the temperature rise calculation. I write that

$$T(z,t) = T_{R.E.}(z) + [T_0(z) - T_{R.E.}(z)] \\ + [T(z,t) - T_0(z)]$$

The radiative heat loss by the matter is then proportional to

$$\beta [T(z,t) - T_{R.E.}(z)] = \beta [T_0(z) - T_{R.E.}(z)] \\ + \beta [T(z,t) - T_0(z)]$$

The time-independent portion of this expression is cancelled by the divergence of the acoustic flux which is a second-order average of the first-order solution to the equations of motion. The radiative heat exchange term which enters the equations of motion and effects phase relations is then the second term on the right hand side which is a first-order. As you say, a large value of β will hold down $T(z,t) - T_0(z)$. However, for $\beta \sim \omega$, the divergence of the acoustic flux is proportional to β , so that $T_0(z) - T_{R.E.}(z)$ is independent of β . Finally, an important point I haven't included in all this is that the coupling constant could be complex. In this case, you get phase lags between compression and cooling. I am almost sure that you will get this in the non-LTE regime.

Thomas — Your work just seems to lead to an awfully big temperature amplitude on the right hand side of the $T - T_{(rad. equi.)}$ equation.

Souffrin — To go back to the problem of wave generation, I would like to make a statement concerning the physical picture for the excitation of the 300 second waves. Where does the instability come from? I understand it as a mechanism which can be traced back to Chandrasekhar and Cowling as a general possible cause of pulsational instability. Its relationship with acoustic modes was clarified by Spiegel and later by Spiegel and Moore. It works the following way. To give rise to that instability, a system needs three kinds of things. It needs a superadiabatic temperature gradient, a mechanism providing a restoring force, and a dissipative process such as heat conduction. A system with these three properties can exhibit pulsational instability. The convective zone has the right temperature gradient. Radiation gives the necessary smoothing of temperature

differences. The extra restoring force can be due to, say, a gradient of molecular weight which enhances the density stratification, or to a magnetic field, or to anything else you want. In the case considered here it is provided by compressibility, i.e., by the acoustic or pressure modes. That acoustic modes provide a restoring force inside a convective zone amounts to the fact that high frequency acoustic modes exist and are stable in such a zone. That is to say, for instance, that one can make noise inside a convective zone.

Let me sketch now how the mechanism works. Suppose an element of material is pushed out of its equilibrium position. Let the restoring force due to pressure prevail over the buoyancy force so that the system is dynamically stable, although the Schwarzschild criterion indicates instability. The parcel is then decelerated and ultimately turns back towards its initial position. Due to, say, radiative transfer, the temperature of the parcel tends to reach the temperature of the surrounding material, so that the parcel experiences a buoyancy force (upwards) at any level, which is smaller when it comes back towards its equilibrium position than when it first went up. Since the restoring pressure forces are not much altered by the heat exchanges, it is immediately seen that, along half a cycle, the balance between the pressure and the buoyancy forces is modified to produce a situation which is clearly pulsationally unstable. My belief is that the one very clear mechanism for producing the 300 sec oscillations is the one considered by Ulrich in his numerical calculations, applied to the convection zone.

Ulrich – The only point I want to make is that resonant acoustic waves are basically pressure modes, where the pressure variations are large compared to the average pressure. In the more familiar gravity modes you have described, the pressure variations may always be neglected.

Kippenhahn – If I understand your mechanism correctly then it is the same which produces overstability when $\nabla = d\ln T/d\ln P$ fulfills the condition $\nabla_{ad} < \nabla < \nabla_{ad} + d\ln \mu/d\ln P$ (μ molecular weight). But then ∇_{ad} is the gradient critical for the problem while you have this puzzling factor 2.

Linsky – I'd like to make a comment to Ulrich about interpreting data, namely spectroheliograms taken in the cores of strong lines. There seems to be a strong tendency for the various elements of the solar chromosphere to segregate themselves into two camps—the brights and the darks. There is no true gray gradation between light and dark regions. I suspect that bright regions, in general, have higher densities and temperatures, and that an instability is indicated by the spectroheliograph data. Namely, a region that is slightly overdense absorbs more acoustic energy and becomes overheated relative to the rest of the chromosphere.

Skumanich — We want an interaction with the radiation transfer people. We want to find what are the key observations which fix the free parameters in the dynamical theory.

Souffrin — I ask for the following observations. The observations are to discriminate between the theories by locating the energy at any level in the atmosphere in terms of frequency and horizontal wave number in the (k, ω) plane. This is not bound directly from observations. But the analysis of the observations in k and ω is the only one useful for the theory.

Skumanich — So you suggest that a variety of lines at different heights should be observed.

Souffrin — Any line at any one height is good, if you can tell us how much energy of oscillation you find, not only at a given frequency, but also at that frequency *and* horizontal wave number. Space-time observations, two dimensional observations, at any altitude are what we need. It would be even better if you could give the density in the diagnostic (k, ω) plane, with amplitude. This would make it possible for us to say if the unstable oscillation of Ulrich is real. If it turns out to be real, it could give us a lot of information about the stratification of the adiabatic gradient, as Ulrich mentions in his paper.

Skumanich — You said that we need simultaneous space, time observations, i.e. in the (k, ω) plane.

Sheeley — He is saying that the meeting place between the observationalists and the theorists is on the (k, ω) plane.

Skumanich — But you can't see this, whether you like it or not you are born in the (x, t) plane.

A. Wilson — Nobody seems to point out that Frazier has already done this. This data already exists.

Souffrin — But we need it even better.

Skumanich — It is unfair to say as good as you can get it, because there are compromises that the observationalists have to make. So we really need to know where one should struggle very hard, and where it's not so essential.

Ulrich — Regarding the (k, ω) plane, I would like to point out that the long horizontal wavelengths are the most important, because these are the ones which penetrate the deepest in the convective zone. I would caution the observers who are looking for evidence for long horizontal wavelengths that they must be sure that they don't have some power at short

wavelengths, where the amplitude seems to be greater, mixed up with their observations. This could be most confusing.

Underhill — Talking about observations to prove a theory, theory is supposed to represent a fundamental behaviour of material. I would like to point out that the non-solar stars are useful. It is not necessary, as far as I have understood the suggested mechanism, that it occur only in a few thousand km long lengths near the surface of the Sun. If the mechanism is universal, it seems plausible that, under conditions on a star with different gravities and radiation fields, the scale may be larger. If so, then you could look at the stars and find brightness variations, in selected wavelength regions, of these short periods. Rather rapid pulsations of certain stars are known. Whether they are relevant to this mechanism or not I don't know. I haven't quite got the physical picture. But I think it is worth exploring. The mechanism might operate under different scales, and then occur on the appropriate stars.

Skumanich — You do give up space resolution when you do stellar observations. If the concern is, in fact, to use the space scales to pin down which of the mechanisms is operating, this could hurt you.

Underhill — I have not understood from the discussion that they have said they need a tiny space scale.

Stein — Two types of observations are possible: a statistical approach which looks for the location of power in the ($K_{\text{Horiz}} \omega$) plane, and the analysis of individual wave packets. Studies of individual wave packets could determine the polarization relationships between Δu , ΔB , ΔP , k , B_0 , as well as the vertical and horizontal propagation velocities, and the shapes and sizes of the packets. The directions of Δu , ΔB , B_0 and k can't be easily determined, but their relative amplitudes, and the variation of the relative amplitudes with height and from center to limb can be observed. Because the magnetic field, B_0 , is more or less vertical in the network in the chromosphere, and because the ratio of Alfren to sound speed changes with height, such studies will give information on the type of wave.

Skumanich — Why do you people give temperature increases, and not the rate of energy dissipation?

Stein — We have that. You would begin to see a temperature rise where substantial dissipation begins, if there were no radiation. Radiation has little effect on the dissipation. The wave is still dissipating the same energy, but that energy is now going into ionization and radiation, not heating.

A. Wilson — The point I got out of today's discussions was that there are two schools of thought about the heating problem. These are either: (a) We need high frequency, short wavelength acoustic waves with very large amplitudes (1-2 km/sec). No mechanism for generating such motions has been suggested and they are not detectable observationally. (b) The main cause of the heating above the minimum is the energy dissipation of the running wave component of the 300 sec oscillation. A small amount of energy in the form of high frequency waves may be needed lower down with small amplitude (.25-.5 km/sec).

These two alternatives bring us face to face in the Sun with a most important contemporary problem in the study of stellar atmospheres. What is microturbulence? As you know every line in the solar spectrum shows an increasing width towards the limb. No theory of line formation predicts this effect. It is always ascribed to microturbulence. The microturbulence has an amplitude of 1-2 km/sec and is on a small enough spatial scale to evade detection by wiggling line bisectors, etc.

Apart from the intrinsic interest of the heating problem, it throws a great deal of light on the subject of microturbulence. If the velocity field postulated in suggestion (a) can be shown to be really necessary to heat the atmosphere we must accept that microturbulence exists. We then have a rather stiff hydrodynamic problem; that of working out how it is generated and propagated. If suggestion (b) wins the day, as I think it will, we have a rather interesting situation. Firstly, the 300 sec oscillation will not give rise to the anisotropic microturbulence required in the photosphere because: its amplitude is too small, its z dependence is exponential, not sinusoidal, and it is a primarily vertical oscillation. Secondly, any small wave motions required to start the heating at low heights will have far too small an amplitude to act as microturbulence.

As you know, the history of microturbulence is very unhappy. It was operationally defined in the days when our understanding of line formation wasn't even roughly correct. Microturbulence is simply a discrepancy factor. Its importance lies in the fact that it plays a central role in methods of determining element abundances.

Now suppose that we can heat the solar atmosphere adequately without using a microturbulent velocity field. What then causes the increase in width of the lines towards the limb? We can look for breakdowns in our descriptive scheme at two points:

- In the theory of line formation: Here we can ask if the assumptions of a frequency independent and isotropic line source function are adequate. Any discussion of these questions must rest on our ability to obtain the radiation field bathing the atom and

the redistribution function for scattering in the atom's rest frame. The problem of obtaining the redistribution function has now been solved. We have discussed that of obtaining the radiation field bathing the atom below: Now let us consider the second independent source of error in our theory.

- Error in the description of the solar atmosphere: Here we can ask the following question: Does the present assumption of a homogeneous atmosphere with anisotropic microturbulent motions provide an adequate description of the inhomogeneous state of the actual atmosphere?

Clearly any attempt to form the radiation field bathing the atom by directly analyzing observational data must solve the inhomogeneity problem first. Recent work of mine has shown that:

- No self consistent explanation of the core profiles of the D lines is possible if the solar atmosphere is homogeneous and does not contain microturbulence.
- No consistent explanation of the center limb variation of the 4571 102 Å of MgI is possible in a homogeneous atmosphere. One is forced to the conclusion that the inhomogeneity of the atmosphere is not well approximated, using the microturbulence model. Therefore the development of the subject should be as follows: (a) Observationally we must obtain sufficient spatial resolution to obtain limb darkening curves at each point in the structure pattern of the inhomogeneity. (b) These limb darkening curves must then be inverted to yield a first order structure. The inversion will assume the simplest line formation physics.

But we have now returned again to the problem of self consistency of the source function (which of course now depends upon position in the atmospheric structure). Only when our data set closes along all resolution axes have we any right to expect adequate agreement between our theory and the observations. Until this time we shall be plagued by non-uniqueness arising from insufficient resolution in wavelength, space or time.

Finally I should like to emphasize again the importance of microturbulence in the solar atmosphere. If it is present, it is by far the dominant part of the velocity field. It does absolutely everything. It looks as if solar hydrodynamicists have already tacitly assumed it does *not* exist, as they have made no attempt to explain its generation or propagation. The majority of the lines used in abundance analysis fall on the flat portion of the curve of growth, and are very sensitive to the value of microturbulent velocity adopted. Until the present confusion about the nature of

microturbulence is cleared up, we can have little idea of the accuracy of the abundances estimated from such lines on the accuracy of our line formation theory.

Skumanich — I would like to suggest a possible experimental, observational test in stars, as Anne Underhill would like us to do, for whether you have a convection driven heating, or a self excited heating, that presumably exists along the main sequence and is not due to the presence of a convection zone as I am guessing. As Wilson suggested, let's look at the diagram of the b-y index versus the absolute power emitted by the CaII chromosphere. This is the actual power output; it is not normalized to the luminosity of the star. You have a curve, for example for the Hyades, that is still rising near where the observations become difficult and disappear. Does this continue to rise? Do we find, very close to here, the rapid turnaround, because the convection zone is disappearing? I don't think the evidence is yet in.

The spectral types here are F6, F7 etc. There is a difficulty in obtaining measurements as we go to earlier stars, because the continuum is rising rapidly. The line itself is being affected by the higher effective temperature, and the ionization changes the line opacity, but we should see the turnaround if it is there.

O. Wilson — I started the Hyades at just an arbitrary point. Perhaps I didn't go far enough towards early type stars.

Skumanich — From looking at your data, I couldn't find evidence of even saturation.

O. Wilson — I think it is because we didn't look there, we didn't go there.

Skumanich — I am then repeating your suggestion that we should look at this end of the main sequence, and see if there is a turnaround where we believe convection is dying out.

Wilson — I think it dies out very rapidly, if you find where rotation ceases.

Underhill — But that doesn't mean that you don't have a chromosphere, because you could have these mechanical pulsations excited in another way. You could have shearing on rotation. You just need some little disturbance in density to have it grow.

Skumanich — The only two mechanisms I have heard about have been the overstability, and the convection zone driving an oscillation field. I don't know much about rotationally driven overstabilities. You may possibly be right. This may not be a test of these ideas. It would certainly be interesting to know whether there is a turnaround or not.

Wilson — You know about this point on the main sequence, which is a (b-y) of 0.28. You no longer see strong chromospheric activity. But Procyon does have weak chromospheric activity. It lies above the cutoff of rapid rotation. If that power point marks the onset, or the end of deep convection, then you still have some chromospheric activity above that, but it is very weak. But we are looking here at a rather narrow range of spectral types. Procyon is F3, and the cutoff point is F4 or F5. As it refers to (b-y), it's a little early.

Skumanich — It's also a subgiant.

O. Wilson — It lies in the main sequence band according to Strömgren.

Skumanich — That's true, but is there evidence that it is going horizontally across?

O. Wilson — This I don't know.

Underhill — This comes back to the problem of defining chromospheres. We've got to stick to the definition of a temperature increasing outwards.

Skumanich — My definition would include my guess that whatever produces calcium emission on the Sun produces it in the main sequence stars of earlier type. I am using an homologous shift of the Sun up and down the main sequence.

Cayrel — Is the Lighthill theory able to predict the magnitude of the mechanical flux of energy coming from the noise in the convection zone?

Skumanich — I have looked at the work in the field, so I will try to answer the question. The Lighthill theory was first done by Proudman and he got a factor in the coefficient on the order of 50. Stein did it again and found that the power put into the tail of the turbulence spectrum governs the coefficient very sensitively. You are going from 50 to 1000 depending upon how you decay the energy in the high k , high ω , part of this diagnostic diagram we have heard so much about. This makes me afraid. When you have an answer that is so sensitive to what you do with the tail of the spectrum, how can we trust the energy estimates? How can we possibly understand the tail of the spectrum, if we don't know the physics of turbulence?

Souffrin — You are quite correct. That theory is a dimensional analysis. It just tells us how much we will modify the output, if we modify the source in some way. That is not very useful for observations.

Cayrel — At least has the flux been computed with exactly the same assumption for a dwarf and a giant, for example?

Skumanich — The problem with the dwarf and giant stars is that in the giants the flow is essentially supersonic. You get into the difficulty that the theory breaks down for Mach numbers close to 1.

Stein — About 4 years ago Strom and I computed the flux the Lighthill theory would predict for a series of main sequence stars and giants. We wanted to see the results of applying the same wave generation assumptions to all the stars. I don't know how you measure the extent of a chromosphere, but we found exactly the opposite from what some of you seem to think is the case. Namely, the ratio of the mechanical energy input to the luminosity of the star goes down as you go down the main sequence to cooler stars, rather than going up. This is why we never published the paper. However, it does go up for the giants, but that is much more uncertain, because you get into much higher flow velocity.

Skumanich — There is no theory for sonic turbulence.

Jordan — I'd like to present the results of some calculations by de Loore. He used the Lighthill theory for generation of sound waves by turbulence in the convection zone of stars. To calculate convection zone models along the main sequence, from A5 to KO, he used the Böhm-Vitense mixing length theory. He found that the hottest, densest coronas could be expected for the late A and early F type stars, with coronal temperatures as high as four million degrees, and electron densities up around 10^{10} . Figure III-16 shows some of his results. The numbers in the left hand graph are effective temperatures; in the right hand graph, they are relative magnitudes for the mechanical energy flux. He normalized things with respect to the Sun, and got for the solar corona a 1.1×10^6 °K corona and $N_e = 10^9$. In order to get that, he had to assume that the flux value was generated only over 10% of the solar surface. He did not include this normalization in his other calculations. His calculation in the convection zone for small τ is inferior to a technique employed by Kyoji Noriai, and, therefore, de Loore tends to overestimate the convective flux, particularly for the earlier type stars where the convection occurs more in the surface regions. I mention this work without any comment, because, in view of all the assumptions and uncertainties in it, it is impossible to evaluate how relevant the calculation is.

Skumanich — What are the observational implications?

Jordan — One of the implications is that one should look at strong ultraviolet lines in coronas of late A and early F type stars. If they do have such hot, dense coronas then you should see these lines. These atmospheres may even be optically thick in some of these lines, due to higher predicted coronal densities, if de Loore is right.

Ulrich — Convection seems to exist in rather early stars, according to de Loore.

Jordan — It is true that, even for stars earlier than A5 and for effective temperature up to $41,000^\circ \text{K}$, de Loore always found some convective instability. However, if you notice the vertical dashed line in Figure III-16 for stars earlier than A5, the region that carried the most convection had a ratio of convective to total flux of less than 20%, and this dropped off

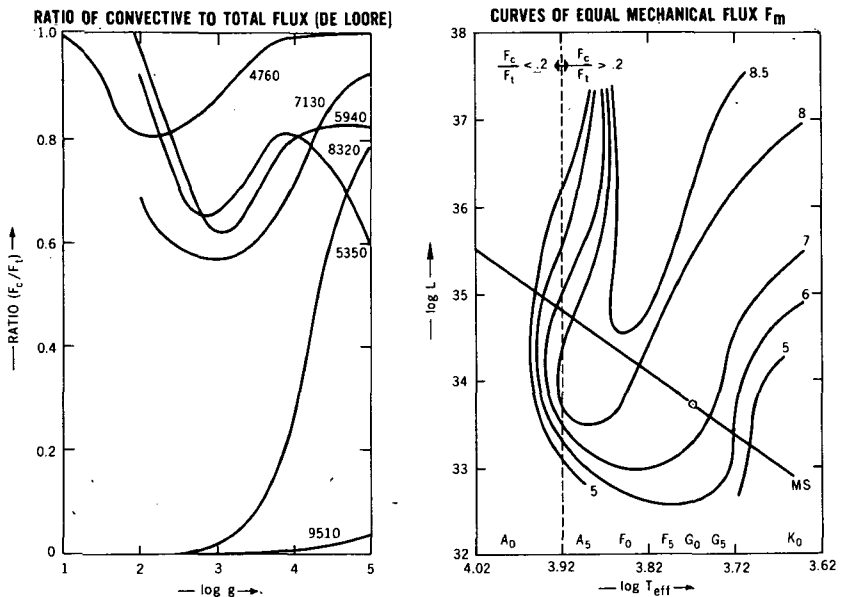


Figure III-16 Ratio of convective to total flux (de Loore)
(left) curves of equal mechanical flux F_m (right).

so sharply that he did not predict strong chromospheric activity for stars earlier than type A5.

Mullan — The results of de Loore, and also the results of Castellani *et al* (Astrophys. & Space Sci. 10, 136, 1971), were computed using the formula $F \sim M^5 v^3$ for the mechanical energy flux. Here, v is the convective velocity and M is the Mach number associated with this velocity. These authors have applied this formula even in cases where M is as large as unity. However this formula was derived theoretically in the limit of small M , say $M < 0.1$, and the accuracy of the formula is expected to become very low as M approached unity. And even if the formula turns out to be accurate, the uncertainties in v due to uncertainties in convection theory are enormously amplified in F . Further

uncertainties arise if magnetic fields are present, so theoretical estimates in mechanical energy fluxes computed in these papers can hardly be considered accurate, even to within an order of magnitude.

Jordan — I agree.

Skumanich — We have to go backwards from the observations to inferences about what is the mechanical flux, and further yet to inferences about what is the convective source. We need more from the theorists in terms of a simple physical picture.

Leibacher — First, I have two comments on my own work. The heating calculation is being done for the Spiegel mechanism right now. It may take a long time. Second, concerning the cause of the heating, Figure III-17 shows how we discriminate between various theories. This is a picture of velocity versus time at a number of different heights where the zero height is the $\tau_{5000\text{\AA}} \equiv 1$ point. There has been some argument

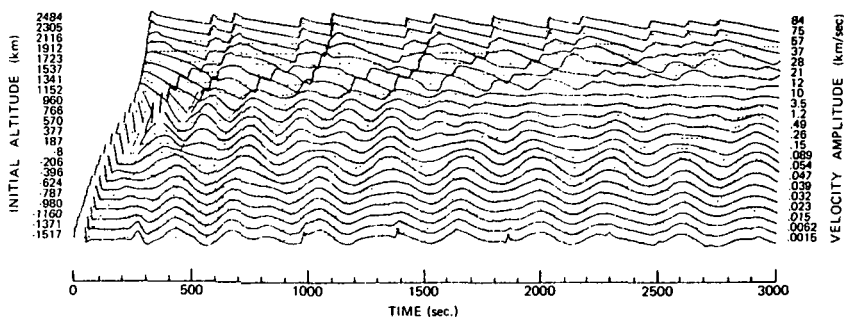


Figure III-17

about there being observational evidence for shock waves in the solar chromosphere. If you look at these profiles you will see that they are very symmetric up through 1000 km, up to the height where Ca K is formed. These are the highest lines we can see from the surface of the earth. Right now it is very difficult for us, with the observed amplitudes here at the earth's surface, to expect to see shock waves in the chromosphere. In Figure III-17 we are looking at the velocity profile from a computer experiment. In some way we create an oscillation which has the correct amplitude here at the surface. This is a 0.2 km/sec oscillation. Now, the question is, as a result of this correspondence with observations, what would we expect to see in the chromosphere? Would we expect to see shock waves higher up? Can we decide on one of the various heating mechanisms? The answer is no; we would not from the surface of the earth. You have to go to the higher lines formed above 1500 km, where

you see the velocity profiles become asymmetric, and the pressure profiles become very narrow. The dotted lines are pressures and you can see here, nearer the base of the temperature rise to the corona, the pressure is very constant. It has a very narrow, in time, over-pressure. Again, those cannot be seen from the surface; we will have to wait for OSO I observations. Now I would like to report on a number of contributions from the informal meeting yesterday afternoon.

Underhill — The temperatures, densities and pressures in the solar chromosphere vary somewhat like those in the atmospheres of early type stars. Only early type stars are much larger than the Sun, so we have a lot more material. It's very well known that you get sporadic emissions in some short period pulsating variables. You also get, as Fischel found, sporadic disappearance of the C IV resonance line. Pulsation like you show may occur in early type stars, and you might not need very much at all to trigger them off. You might not even need a convection zone to start them. But the result of those shocks is superheating. I don't like the idea of saying chromospheres exist only for stars with convection zones.

Leibacher — I would first like to consider two sets of observations by Musman and Beckers on the presence of exploding granules in the surface layers of the Sun. For a long time there has been a series of observations by Rösch of the appearance of very bright spots on the solar surface; which then expand into a ring and disappear. These are continuum observations. With the new Tower Telescope at Sacramento Peak, Musman has made movies of these appearances, and has made some hydrodynamic models of them which are very similar to cumulus clouds models. Beckers has been doing similar observations with his velocity filtergram system. It has velocity pictures and short velocity movies, and hopefully in the near future longer velocity movies of these exploding granules. To the extent to which oscillation and heating theories depend upon excitation by granulation, I think all of a sudden we are moving ahead very rapidly.

However, it should be noted that recent work of Sheeley and Bhatnagai indicates that the granulation and oscillation horizontal scales differ by a factor of three.

A second area of discussion was the observations of the 5 min. oscillations on the solar surface, and the reliability of these observations. Figure III-18 is for those who have been talking about the horizontal scales that are involved here. This is the famous diagnostic diagram. The isophotes here are iso-power lines and are the results of some observational work by Frazier. A great deal of effort has been placed on trying to understand the double peak nature of the oscillatory motion. There are two distinct peaks. If you look at a power spectra, Fourier analyzing the

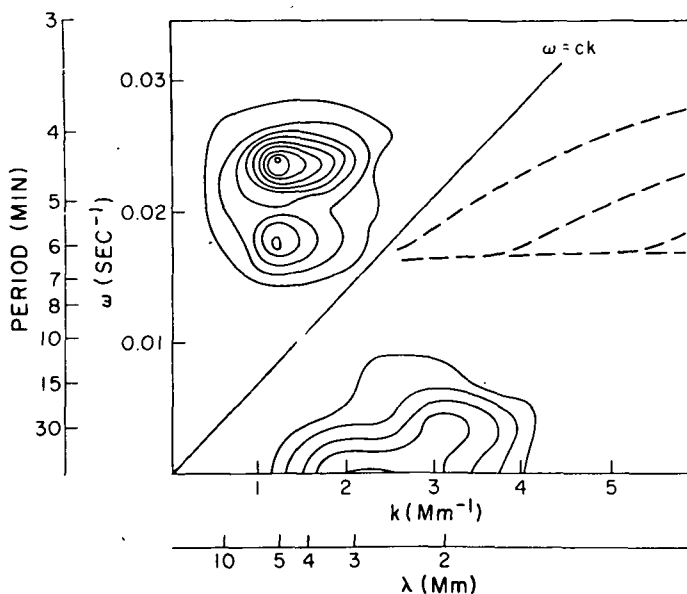


Figure III-18

velocities, compressing everything onto zero horizontal wave number, power as a function of frequency shows a number of very narrow peaks which correspond to very long coherence for oscillations. Figure III-19 is a very long record obtained at Mt. Wilson by Howard which has been analyzed by Cha and White at HAO. You see here, for instance, what

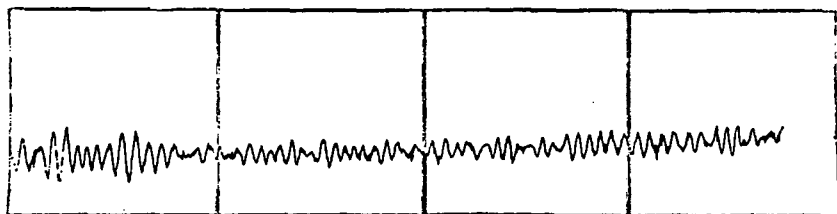


Figure III-19

appears to be an extremely long, in phase, series of oscillations. It is the length of that packet, then, that gives rise to the very narrow peaks in the power spectra. A lot of effort has gone into the interpretation of the multiple peaks and their positions. The result is now emerging from White and Cha, and separately from Deübner, that the very peaked nature of the power spectrum is a result of statistical uncertainty in the records.

There aren't enough independent data points. More satisfying to White and Deibner are single peaked envelopes, which are stable in time. There is bound to be some reluctance in accepting a change as drastic as this. One of the most convincing arguments in favor of it is White's ability to reproduce the observations in the statistical sense, from introduction into a power spectrum such as this of purely random noise. In other words, he can take a filter and filter noise and get the observations back out. I have some of White's pictures. His work will be published shortly. In summary of their findings: the best representation they have for the observations of the 5 min. oscillations are that it is a narrow band random process with the emphasis placed upon the randomness.

As a last point, there has always been some debate between the granulation excitation, as I have mentioned, and over-stability arguments such as Ulrich, and Stein and I have been proposing. If you look at the structure of the individual packets, you find you can get essentially whatever you want. If you look at frequency vs. time in a packet, some packets have their frequency increasing with time, until the packet disappears; others have the frequency decreasing with time. If you look at the amplitude vs. time in a packet, in other words, and ask if the packet starts off very large and then dies out, you find just that. You also find the same result with time running in the opposite direction. So any theory you want to justify, you can find a section of the record that will reproduce it. If you take enough of the record a statistically significant sample of the oscillation, all the determinism drops out.

For the theory of the 5 min oscillation, there exist two primary schools of thought. One is currently represented by the people at the University of Rochester, Al Clark and John Thomas, who have been proposing that the oscillation consists of trapped internal gravity waves. Internal gravity waves are essentially buoyancy waves, to some extent similar to the waves one sees on the surface of the ocean. They are trapped by the temperature structure of the solar temperature minimum. The other school, represented by Ulrich, Stein and myself, sees the energy concentration of the 5 min oscillation as being sub-photospheric, and the model is more represented by an organ pipe, the upper surface of which is the top of the hydrogen convection zone. The lower surface is several mega-meters beneath the surface. The observations in fact, relate to evanescent waves, non-propagating waves that are tunneling through the temperature minimum.

Skumanich — In view of its importance, I would like to reopen a discussion of the convection-zone.

E. Böhm-Vitense — Unreasonable results are obtained by people who apply the mixing-length theory in cases where the convection zone is

thinner than the mixing length. It doesn't make sense to take a mixing length equal to the scale height, and obtain a convection zone which is only one half a scale height thick. Second, the mixing-length theory, as it stands, is certainly not the ideal theory. I think we can estimate the velocities without relying on the theory. If at some point, essentially the whole energy is transported by convection then by putting the convective energy transport equal to the total energy transport you can derive the average velocity at this point without too much uncertainty. There is less than a factor of two uncertainty in the velocity which you get this way, as long as you are sure that at that point the total energy is transported by convection.

Skumanich — Does this mean that the velocities become large.

E. Böhm-Vitense — They do increase with lower densities, which means they increase with increasing temperature or increasing luminosity until the convection becomes ineffective. On the main sequence, that happens at about 8000 °K.

Peytremann — I have a question about the graph of de Loore which shows the ratio of convective flux to total flux: to what depth do they refer?

Jordan — It varied. Certainly for the late A and early F stars, it was above mean optical depth unity.

Stein — What we calculated, when Strom and I did it, was not the ratio of convective but of mechanical energy flux to luminosity deduced using the Lighthill theory.

Peytremann — The mixing length theory you all use can be criticized, but whether it is wrong or not, it should lead to the same results when used by various people. The small ratio of convective to total flux in de Loore's graph (Figure 1 (Jordan) left hand graph) may result from the fact that this theory is certainly not valid when applied to layers thinner than the mixing length itself, as may have been the case for the hotter stars de Loore treated.

Skumanich — I think that the idea of using a scale length the order of a scale height is not crazy at all. In fact, my own work in 1955 shows, this was for convection in a polytropic atmosphere, and that as you change the horizontal length scale, the flow packs itself into that scale height which is like the horizontal size. You fix this size, as Böhm has found out, by damping effects. We are still investigating whether or not there is convection in the early type stars.

Peytremann — In all the models I have calculated, I never had any convection in atmospheres earlier than spectral type A.

Kandel — The question is what are you using mixing-length theory for. If you are talking about energy transport, which is internal energy flux, then the mixing-length approach may be satisfactory; it may give reasonable results; and you get velocities out of that which are certain average velocities which work very well. For the purpose of computing a mechanical energy flux which will perhaps heat a chromosphere and corona, you have a very different type of average over the velocity. You are working with the tail of the distribution. I don't think the mixing-length people would say that they could tell you what the tail of the distribution will be, when it is involved with some average over velocity to the eighth power (as Mullan said). You have this enormous uncertainty which makes it very very hard to believe any of these predictions.

Skumanich — I agree.

Mullan — Just how accurate are these models? The fact is that we do not know the run of Mach number with depth in any star. We do know that certain M dwarfs (the flare stars) have coronas, for they have been observed to have radio bursts somewhat similar to Type III solar bursts. Kahn and Gershberg have found that gas densities in the coronas and chromospheres of the flare stars are up to 100 times greater than in the Sun. However, the maximum convective velocity in an M dwarf is expected to be smaller than in the Sun according to current convection theories. If this is so, then a star with small convective velocity is somehow able to generate sufficient mechanical energy to support a corona 100 times denser than that in a star with a higher convective velocity.

Skumanich — That's a good point. We certainly have avoided the variation of model and dynamical properties along the main sequence. I think part of that is that we don't have a full understanding of the observations. The observations exist, thanks to Wilson.

Stein — It may be that turbulent noise generation by the Lighthill mechanism, while present and important for heating the chromosphere, is not the primary source of mechanical energy for heating the corona. The calculations of Leibacher and myself suggest that the 5 minute oscillations are heating the corona. Such long period waves will get their energy up to the corona more easily. We are in the process of calculating the generation of the 5 minute oscillations by thermal instability in the superadiabatic convection zone. This process will presumably have a different dependence on stellar properties along the main sequence than the Lighthill mechanism.

Underhill — There is a theory that suggests that stars with magnetic fields are rotating underneath. These magnetic fields will become wound up and

can become very strong. This theory explained how to get a magnetic field in a white dwarf. At the same time, the star blew off its atmosphere, so you had the white dwarf left over. If the white dwarf has a thin amount of cool expanded atmosphere around it, it will give you a continuous spectrum. If those magnetic fields somehow accelerate the material, you will get x-rays. Perhaps some of the highly excited atmospheres are not heated by mechanical energy, but may be heated by x-rays which we cannot observe because of their attenuation between the source and the object. It's not impossible.

Manich — I think, by arguments of homology, that along the main sequence the fields can be ignored. The observations of the solar wind, which is driven by the energies deposited in the corona, seem to be independent of the magnetic cycle of the Sun. I am not sure that this implies that they are secularly independent. All we know is that they don't follow the actual oscillation. But they may follow the mean amplitude of the magnetic field. Whether the field can act as the energizer of the gas, as you suggest, I leave to the white dwarf men. However, other flares represent, in some generalized sense, some heating mechanism for the corona, I think that that would also be cycle dependent, which we don't observe.

Alan — Observers cannot depend on theoreticians for guidelines as to what should be observed, simply because uncertainties in the theory of mechanical energy generation are so great. In fact the problem must be inverted; and I would like to ask the observers to present theoreticians with a value for the solar mechanical energy flux deduced from observations. Theoreticians might then profitably use this as a constraint on the various free parameters at their disposal.

Manich — One problem is that some of the theoreticians give us a model for the dissipation as a function of height in the atmosphere, while others give us temperature and density models, but the two don't spend enough time checking each other. One might say that the atmosphere is a filter and what we really want is the pass band of the filter.

Offrin — I would like to suggest that people not look too closely at the observations. Many excellent theories in science would not have been developed had people had very detailed observations. A number of large scale effects on the Sun which have been discovered would not have come to light if people had been concerned only with more detailed observations. This is not to say that I advocate no observations, but only that I think the theory should be better developed so that we at least understand the large scale phenomena.

Skumanich — So long as we always keep in mind that, in the absence of laboratory experiments, theory and observations must bootstrap each other in astronomy if we're to understand anything at all.

Schwartz — Concerning observations, I would just like to emphasize that observations of velocity fields are not observations of the power which is propagating through the atmosphere. The only way to learn if there is energy propagating from velocity field measurements is to measure the phase relations between pressure and velocity. If these two quantities are in phase, then you know energy is propagating. If the pressure and velocity are 90 degrees out of phase, then it doesn't matter how large the velocity you have is, you aren't propagating any energy. So what we fluid dynamics people need is for the radiation transfer people to solve the transfer problem to give us information on the pressure from the intensity variations. I know this is a tall order, as it means doing the transfer problem many times (10 or 12) during the 300 sec period, rather than once; but it is what we need.

PART IV

**VARIATION OF CHROMOSPHERIC
PROPERTIES WITH STELLAR MASS AND AGE**

Chairman: Lawrence Aller

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CHROMOSPHERIC ACTIVITY AND STELLAR EVOLUTION

Rudolf Kippenhahn
Göttingen University Observatory

STELLAR EVOLUTION AND MECHANICAL FLUX

Stellar evolution carries a star through the Hertzsprung-Russell-Diagram. For a given mass, M , one obtains both luminosity, L , and radius, R , as functions of time, t . From these parameters we determine the surface gravity and the effective temperature as functions of t :

$$g(t), T_{\text{eff}}(t).$$

It is these two parameters which determine the properties of the outermost layers of a star, the atmosphere and the top of the hydrogen convective zone.

From the equations of mixing lengths theory (Böhm-Vitense, 1958) one can derive for the Mach number, M , that:

$$M^2 < \frac{1}{8} \frac{\ell_2}{H_p^2} (\nabla - \nabla_{\text{ad}})$$

where ℓ = mixing length, H_p = pressure scale height and $\nabla = d \ln T / d \ln P$, $\nabla_{\text{ad}} = (d \ln T / d \ln P)_{\text{ad}}$. We thus see that the Mach number can approach 1 only in regions where $\nabla - \nabla_{\text{ad}}$ is large, that is in those regions where the stratification is highly superadiabatic — as it is at the top of the convective zone. Sound waves can be formed in these layers only; thus the mechanical flux also depends only on $g(t)$ and $T_{\text{eff}}(t)$. Therefore, for the determination of the mechanical flux a grid of models of stellar outer layers as functions of the two parameters g, T_{eff} is necessary.

Recently de Loore (1970) has computed the flux for a set of model atmospheres. The mechanical flux F_{mech} which he derived is given in the $\log T_{\text{eff}} - \log g$ - plane of Figure IV-1. As has been said yesterday, de Loore's models exaggerate the mechanical flux for those models which have convective zones thinner than the mixing lengths. In the next three figures evolutionary tracks are plotted in the $\log T_{\text{eff}} - \log g$ - plane together with de Loore's mechanical flux areas. In Figure IV-2 the pre-main sequence evolution as well as the post main sequence evolution up to helium flash are plotted for a star of one solar mass. One can see that the star is always in the region of strong mechanical flux. This holds also

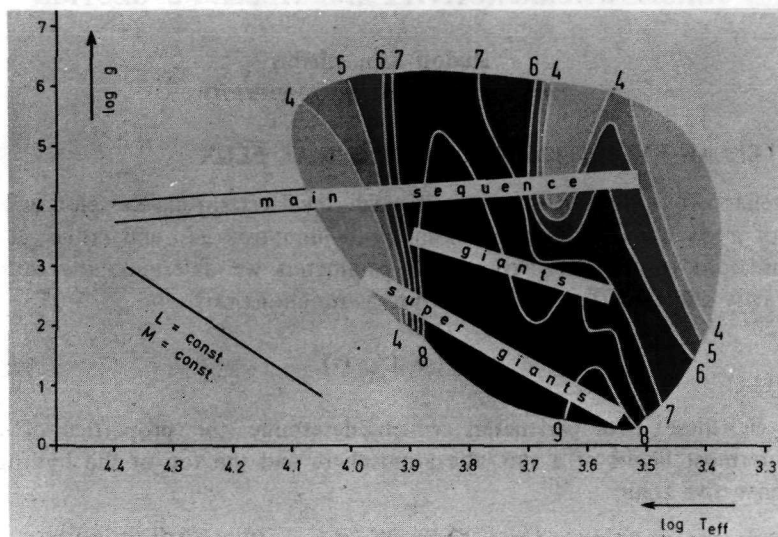


Figure IV-1 The mechanical flux F_{mech} as a function of g and T_{eff} computed by de Loore (1970) with the Lighthill-Proudman theory. The numbers at the white lines give $\log F_{\text{mech}}$ where F_{mech} is in c-g-s units. The straight line in the lower left corner gives the slope of an evolutionary track which is horizontal in the HRD.

for the post main sequence evolution of a 1, 3 solar mass star (Figure IV-2). Stars of 1, 3 solar masses settle down on the main sequence near F5. This is the region where on the main sequence one observes the transition from stars with Ca emission to those without. One therefore is surprised that according to de Loore's computations such a star is right in the middle of the region of strong mechanical flux. One would expect the star to be on the left border of the area of strong mechanical flux instead. This is probably due to the enhanced mechanical flux in the thin convective zones in de Loore's computations. Figures IV-3 and IV-4 show that stars of higher masses start on the Hayashi track in the region of strong mechanical flux, move into the low flux region and then come back into the high flux region during central helium burning and further later evolution. While the more massive stars make loops they go several times from high flux to low flux regions and vice versa.

It has been indicated during this conference that the mechanical flux computed according to the Lighthill-Proudman theory is not very reliable due to uncertainties in the theory of convection. We were confronted yesterday with at least two new and different possible mechanisms of heating. Certainly these mechanisms have to be worked out more thoroughly before one can decide whether we really have the correct

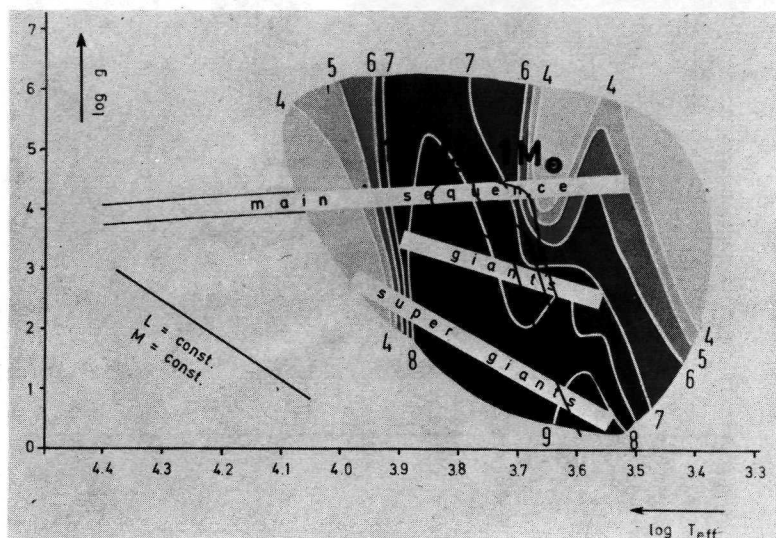


Figure IV-2 Evolutionary tracks for $1 M_{\odot}$ and for $1.3 M_{\odot}$ in the $\log g$ - $\log T_{\text{eff}}$ plane. The $1 M_{\odot}$ star starts in the lower right corner, moves into its pre-main sequence evolution towards the main sequence and goes back into the lower right corner in the post ms evolution. For the $1.3 M_{\odot}$ star only the post ms evolution is plotted.

theory of mechanical heating. It is, for instance, not sufficient to show that a certain type of motion is unstable by making only a linear analysis.

What one has to show is that such an instability, if it is fully developed, has sufficient energy to produce the mechanical flux necessary for chromospheric heating. In the case of convection we know that in many stars all the energy of the star is transported through such motion and it is therefore easy to get the required energy from convection. It should be kept in mind that in the HRD the observed transition from stars with observed calcium emission to those where calcium emission is not, or is only seldom, observed seems to agree fairly well with a line of constant mechanical flux generated by convection.

In particular on the main sequence there is a sharp transition between calcium emission and no calcium emission (as it is observed by O. C. Wilson, 1964) which coincides with the well known transition from convection to no convection. Since the flux depends on the eighth power of the turbulent velocity one would expect a sharp cut-off in the mechanical flux at this transition. That this cut-off is not so pronounced in Figures IV-1 to IV-4 may be due to de Loore's treatment of thin convective zones.

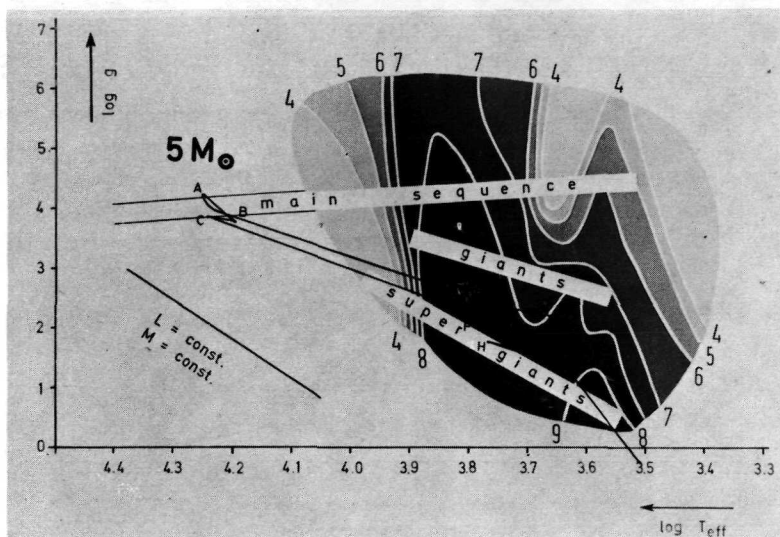


Figure IV-3 The evolutionary track for $5 M_{\odot}$ from the pre-ms evolution to the ms. Central hydrogen burning starts at point A and is terminated at point B. Further evolutionary stages go from C to H.

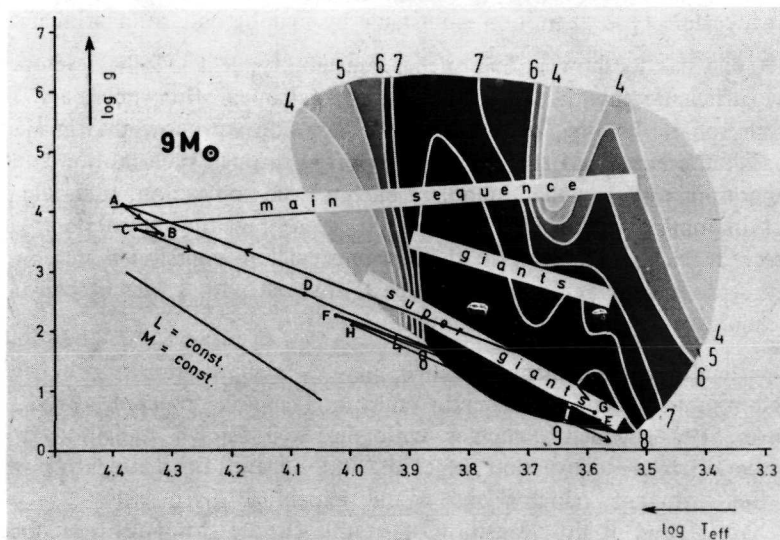


Figure IV-4 The evolutionary track for $9 M_{\odot}$ from the pre-ms evolution to the ms. Central hydrogen burning: A-B/further evolutionary stages: C-H.

INFLUENCE ON STELLAR EVOLUTION OUTER BOUNDARY CONDITIONS

I do not think that stellar models would be drastically different if the normal grey or nongrey atmospheric boundary conditions were replaced by a fit to an outer layer with a more complicated temperature profile. Only cool stars are sensitive to their outer boundary conditions – but only in the sense that their radii and therefore their position in the HRD is dependent on boundary conditions.

But the evolution itself is steered by the very deep interior and the interior of an evolved star does not know about the envelope.

MASS LOSS BY STELLAR WIND

The mass per year blown into space by the solar wind is small. It is less than the decrease in mass of the Sun due to the mass equivalent of its radiated energy. From the point of view of stellar evolution this mass loss can therefore be neglected. According to Weyman (1962) α Ori has a mass loss of

$$\frac{dM}{dt} = 4 \times 10^{-6} M_{\odot}/\text{yr.}$$

α Ori is a star of about 20 solar masses in its post main sequence evolution. In the most favorable case this mass loss might add up during central helium and carbon burning to a mass loss of a few percent for that star.

The luminosity of a main sequence star is reduced by mass loss according to the mass-luminosity relationship. But a star with shell burning remains at the same luminosity even if 90% of its hydrogen rich envelope is removed. This is well known from computations of mass exchange in close binary systems. Therefore it is very difficult to decide from observations whether an evolved star has undergone mass loss.

This is the reason why for years an argument has been going on between the non-linear cepheid pulsation theory people on the one side and the evolutionary and linear pulsation theory people on the other side. Christy (1968) claims that he can get agreement with observed light curves only if he assumes that cepheids have but half of the mass given by the normal evolution theory. On the other hand Lauterborn, et al., (1971), give an evolutionary track for a $5 M_{\odot}$ star which has loops in the red giant region with several slow crossings of the cepheid strip. They found that if more than 5% of the mass of the star were taken off the envelope, the loops disappear. Therefore, they argue, if mass loss takes place there would be

no slow crossings of the cepheid strip, there would then be no cepheids and Christy would then have no observed light curves to compare his theoretical curves with.

Since the mass of the cepheids is still undetermined (Fricke, Strittmatter, Stobie, 1972, Cox, King, Stellingwarf, 1972) if we wish to understand whether mass loss from coronas influences the evolution of stars we certainly have to look for the masses of the cepheids since this offers a chance to obtain information.

LATE PRE-MAIN SEQUENCE AND MAIN-SEQUENCE-EVOLUTION AND CHROMOSPHERIC ACTIVITY

When O. C. Wilson (1963) found that field stars have less chromospheric activity than the same type of stars in galactic clusters a completely new point of view came into play. Imagine: stars at the same place in the HRD and (since they are, therefore, also on the main sequence) stars of the same mass, differ in their Ca + emission! These stars should have the same atmospheres since g and T_{eff} are the same. They certainly have the same mechanical flux if it is computed in the same way as de Loore, but they differ in their chromospheric activity. The puzzle would remain even if one of the two new mechanisms mentioned yesterday were to replace the mechanical flux due to sound waves coming out from the convective zone. All those mechanisms would produce the same mechanical flux for the same values of g and T_{eff} .

Kraft (1967) found the correlation between chromospheric activity and rotational velocity. Now we know from the work of Skumanich (1972) that roughly

$$\text{Ca}^+ \text{-emission} \sim \Omega \sim t^{-1/2}$$

where Ω is the angular velocity of the surface. From the Sun we know the Ca⁺ emission is correlated to the magnetic field. Beckers and Sheeley during this conference told me that for fields between 0 and 100G there is a positive correlation between Ca⁺ emission and the magnetic field strength $|B|$ although there is a large scatter around this relationship. Finally, we know that the solar magnetic field is related to the rotation of the Sun. We therefore come up with the following logical scheme, as shown in Figure IV-5.

The outer five boxes, forming a pentagon, give the logical structure as it follows from the first two sections of this article. Stellar evolution changes effective temperature and surface gravity of the stars, and these two parameters determine the top of the convective layers in which the

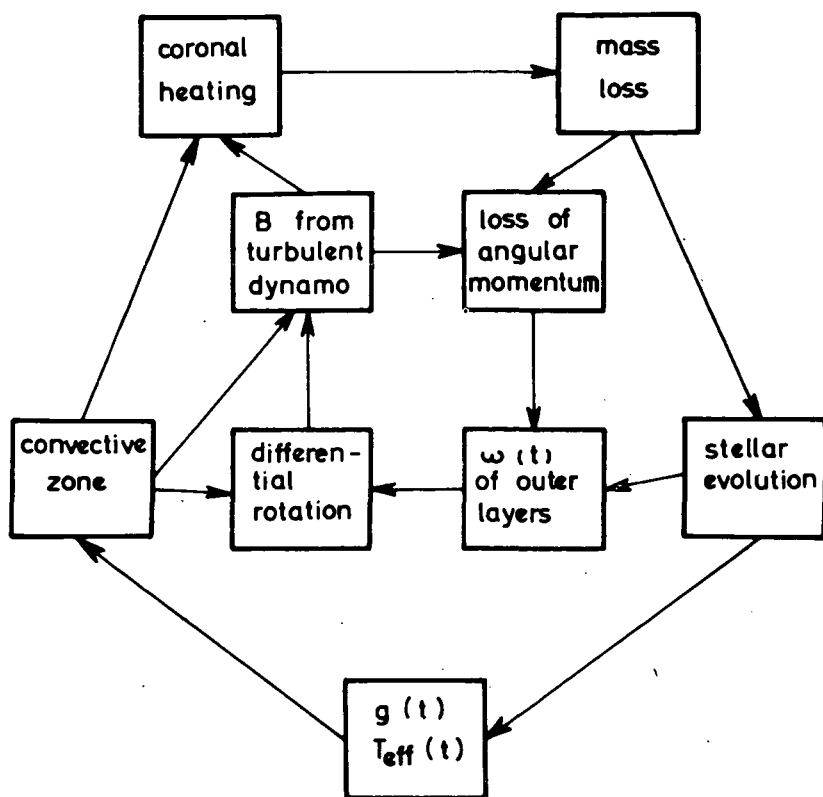


Figure IV-5 The logical structure which connects stellar evolution with nonthermal heating of chromospheres coronas.

mechanical flux is generated which heats the outer layers. Heated outer layers may produce a stellar wind which may influence the stellar evolution.

Due to the effects mentioned in this section one must also take into account the inner boxes. We know much less about these boxes inside the pentagon. What seems to go on inside the pentagon is more secret to us. As you see in the figure, almost all the arrows, that is all the information, goes into the interior of the pentagon and almost nothing comes out. But there is one leak.

If rotation is taken into account we must keep in mind that during stellar evolution when the star is contracting or expanding the angular velocity distribution will change. The angular velocity Ω , near the surface might therefore also be influenced by stellar evolution. For the Sun there is an indication that convective zones show differential rotation. Differential

rotation together with convection can produce magnetic fields which on the one hand can enhance the outcoming magnetic flux and especially can determine the region where the dissipation takes place. It therefore influences the heating of the outer layers. On the other hand the stellar wind together with magnetic fields can produce a strong loss of angular momentum which, together with stellar evolution, influences the angular velocity distribution of the star. In the following we will discuss in more detail the interior of the pentagon.

EVOLUTION AND STELLAR ROTATION

Even if we assume that the star does not lose angular momentum the problem is difficult. We do not know how effective mechanisms, such as large or small scale motions or magnetic fields, are at redistributing angular momentum in the stellar interior. We do know that only very restricted angular momentum distributions are stable, but we do not know what the time scales of some of the instabilities are and whether they are really important during the life time of a star.

If we knew the true theory of the flow of angular momentum inside the star during evolution, the surface angular velocity would be known as a function of time: $\Omega = \Omega(t)$. From numerical calculations with different assumptions about the redistribution of angular momentum during stellar evolution Kippenhahn, Meyer-Hofmeister, Thomas (1970) one can derive, as a very crude thumb rule, that

$$\Omega(t) \sim \frac{1}{R^2}(t)$$

This relationship is valid in the case of local conservation of angular momentum. It turns out that this is a fairly good approximation in the physically more realistic case when one assumes that the hydrogen convective zone rotates as a solid body and that in the radiative regions angular momentum is locally conserved.

For our purpose in this review it is not so important to know the numerical details but rather to understand the logical structure, that is to find out what determines what. For this purpose it is sufficient to know that, when stars from the main sequence evolve into the red giant region, the surface angular velocity goes down roughly as indicated by the above formula. Observed rotational velocities for red giants (Oke, Greenstein, 1954) support the above formula.

DIFFERENTIAL ROTATION

Winding and unwinding of magnetic fields seems to be important for the solar dynamo. Therefore differential rotation is essential. The turbulent viscosity of the hydrogen convective zone gives a time scale of only 100 years for adjustment. The differential rotation therefore is certainly not a fossil relic from earlier phases of evolution. It must be maintained by some unknown mechanism.

Many attempts have been made to explain the solar rotation law. I think everybody now agrees that it is a pure hydrodynamic phenomenon; that the magnetic fields there have to follow the gas in the hydrodynamic flow and do not influence the rotation. This is indicated by the fact that the differential rotation does not vary with the solar cycle during which the magnetic field changes sign.

Among the hydrodynamic approaches there is that via non-isotropic viscosity proposed by L. Biermann (1951), Kippenhahn (1963) and Köhler (1970). This approach did not encounter much enthusiasm from the professional hydrodynamicists. On the other hand there are the attempts by Busse (1970) and recently by Gilman (1972).

In the present we do not know if any of these approaches will really turn out to be true. But, for the moment, we can just assume that convective regions like to rotate differentially – whatever the reason is.

TURBULENT DYNAMOS

During the last years, theories for the solar cycle have been developed by Babcock (1960) and Leighton (1969) and also by Steenbeck and Krause (1966). Both approaches have in common that turbulence and rotation are considered in a statistical theory which yields equations for the mean velocities and for the mean magnetic field. These equations contain terms in addition to those of ordinary magnetohydrodynamics due to correlations in the turbulent quantities. In normal magnetohydrodynamics one has

$$\frac{\partial \mathbf{B}}{\partial t} = \frac{c^2}{4\pi\sigma} \Delta \mathbf{B} + \text{curl } [\mathbf{V} \mathbf{B}]$$

where \mathbf{B} , \mathbf{V} , are the magnetic field, the velocity field and σ the electric conductivity. The first term on the right hand describes the dissipation of magnetic energy due to ohmic losses while the second term alone would give the frozen-in condition. From Cowling's theorem there follows that in the axisymmetric case a given velocity field \mathbf{V} can not maintain a magnetic field against the dissipation. But in the case of turbulent motion

one obtains an equation similar to that above for the mean field but this equation contains an additional term as indicated in the lower part of Figure IV-6. This additional term has been derived by Steenbeck and Krause, and it contains the fact that rising and falling turbulent elements

LEIGHTON'S NONLINEAR MODEL:

| | |
|--|--|
| $\frac{\partial B_\varphi}{\partial t} = \underbrace{\sin \varrho \left(B_\vartheta \frac{\partial \Omega}{\partial \vartheta} + r B_r \frac{\partial \Omega}{\partial r} \right)}_{\text{Winding of Frozen-in Field}} - \underbrace{\delta \cdot \text{const.} B_\varphi B_\varphi}_{\text{Depletion Due to Eruption}}$ | $\delta = \begin{cases} 0 & B_\varphi < B_\varphi \\ 1 & \text{otherwise} \end{cases}$ |
| $\frac{\partial B_r}{\partial t} = \underbrace{\frac{1}{T_0} \frac{\partial}{\partial \cos \varrho} \left[m^2 \varrho \frac{\partial B_r}{\partial \cos \varrho} \right]}_{\text{Diffusion}} - \underbrace{\delta \cdot \text{const.} \frac{\partial (B_\varphi \cos \varrho)}{\partial \cos \varrho}}_{\text{Creation of } B_r \text{ by Tilt "}}$ | |
| $B_\vartheta \text{ from } \nabla \cdot \underline{B} = 0$ | |

KRAUSE-STEENBECK'S LINEAR THEORY:

| |
|--|
| $\frac{\partial \underline{B}}{\partial t} = \underbrace{\text{const.} \Delta \underline{B}}_{\text{Diffusion}} + \underbrace{\text{curl.} (\underline{V} \times \underline{B})}_{\text{Winding of Frozen-in Field}} + \underbrace{\text{curl} (\alpha \underline{B})}_{\alpha\text{-effect}}$ |
| $\nabla \cdot \underline{B} = 0$ |

Figure IV-6 Formulae for the two types of models for turbulent dynamos.

are forced to a helical motion by Coriolis forces. The magnetic field is tilted by these elements in such a way that the mean field behaves as if there is a mean electric current parallel to the mean magnetic field (α -effect). This effect was already indicated by Parker (1955). The papers by Krause, Rädler and Steenbeck on the turbulent dynamo have recently been translated by Roberts and Stix (1971) (See also Deinzer, 1971).

Similar additional terms have been introduced into the magnetohydrodynamic equations by Leighton as one can see in Figure IV-6. In this theory similar to the α -effect of Steenbeck and Krause a "tilt" is assumed when a pair of sunspots appear when a magnetic "rope" comes to the surface.

The Babcock-Leighton theory is non-linear and one therefore obtains for any given angular velocity distribution, and for a given differential rotation, a magnetic field configuration. Recently Durney and Stenflo (1971) have investigated the strength of the magnetic fields in Leighton's dynamo in dependence on the angular velocity assuming the differential rotation to be the same. They found that the magnetic field is approximately proportional to the angular velocity.

$$|\underline{B}| \sim \Omega$$

New solutions of the Steenbeck-Krause equations have recently been found by Köhler (1972) who used a solar model with a realistic convective zone, and derived from the properties of the convective layer the factor in front of the $\Delta \underline{B}$ term of the Steenbeck-Krause equation as well as their α as functions of depth. He indeed obtained periodic solutions. Certainly the linear theory can not give amplitudes. But Stix (1972) investigated the case of non-linear limiting which would set in if the amplitudes become sufficiently high. Then, as already suggested by Steenbeck and Krause, the magnetic fields would be so strong that they would react on turbulence and inhibit the helical motion. With such a cut-off he found that the amplitudes roughly go like

$$|\underline{B}| \sim \Omega^{3/2}$$

The theory of the solar cycle is incomplete but one might already dare to make some predictions for other stars. If stars have a convective zone and are rotating, one would expect that they also have differential rotation. In this case the turbulent dynamo may work and one would expect the magnetic field to increase with rotational velocity if everything else including the degree of differential rotation is kept constant.

LOSS OF ANGULAR MOMENTUM

It had first been pointed out by Schatzman (1954) that mass loss from a rotating star with a magnetic field gives a high loss of angular momentum. This is due to the fact that the outstreaming material gains angular momentum from the magnetic field until it has reached a point where it is released into space. Following Weber and Davis (1967) one can write

$$\frac{d}{dt} (k M R^2 \Omega) = \frac{2}{3} r_A^2 \Omega \frac{dM}{dt} \quad (1)$$

$k M R^2$ is the inertial momentum of the star. The factor k can be computed for any given stellar model. The radius r_A is the distance from

the star at which the Cowling number

$$C^2 \equiv \frac{v_r^2}{B_r^2 / 4 \pi \rho}$$

is one. Here v_r and B_r are the radial components of velocity, and magnetic field. If we follow recent work by Durney (1972) in a more generalized way one can show that

$$r_A \sim B_0 v_A^{-1/2} \left(\frac{dM}{dt} \right)^{-1/2}$$

where v_A is the value of v_r at the point where $C = 1$ and B_0 the field at the surface of the star. If we assume from the dynamo theory it follows that $B_0 \sim \Omega \gamma$ we can then write

$$r_A \sim \Omega \gamma v_A^{-1/2} \left(\frac{dM}{dt} \right)^{-1/2}$$

From equation (1) it then follows that — as long as the radius of the star is not varying with time, as it is in the case for the main sequence stage to a high degree of approximation, one can write

$$\frac{1}{\Omega} \frac{d\Omega}{dt} = \text{const.} \quad \frac{\Omega^2 \gamma}{v_A}$$

Therefore for any given rate of mass loss and for any assumption as to how the radial velocity, v_A , varies with time, one can determine the angular velocity as a function of time. Probably v_A as well as dM/dt will vary with the angular velocity since the angular velocity will enhance the turbulent dynamo and therefor enhance the heating and therefor the mass loss. Generally one can assume

$$v_A \sim \Omega^\xi$$

with a free exponent ξ . Then equation (2) can be integrated and gives (as long as $\xi \neq 2\gamma$)

$$\Omega = \text{const.} (t - t_0)^{\frac{1}{\xi - 2\gamma}}$$

Durney has used this formula for the special case $\xi = 0$, $\gamma = 1$ in order to obtain Skumanich's law $\Omega \sim t^{-1/2}$. Certainly one must know more about the mechanisms inside the pentagon of Figure IV-5. The main purpose here

is to show that, in principle, the time dependence of the angular velocity distribution is determined.

We have now discussed the boxes inside the pentagon and I must say I have the feeling that the whole logical structure indicates quite a closed picture although many details still have to be worked out.

TURBULENT VELOCITIES IN THE ATMOSPHERES OF ROTATING STARS

I would like to add a comment on the question of hot main sequence stars where convective theory gives practically no turbulent velocities. It has been shown by Baker and Kippenhahn (1959) that near the surfaces of rotating stars meridional circulation can reach fairly high velocities. I will give a different approach here. We consider very rapidly rotating stars where, near the equator, the centrifugal force almost balances gravity. Then it follows from von Zeipel's theorem that the effective temperature at each latitude is connected with the effective gravity:

$$T_{\text{eff}} \sim g^{1/4}.$$

It follows that pressure and temperature are constant on equipotential surfaces for hydrostatic equilibrium. But when we try to construct atmospheres in each latitude it turns out that the mean optical depth τ is not constant on equipotential surfaces $\phi = \text{const}$:

$$d\tau = -\kappa d\tau = -\kappa d\phi/g, \quad \kappa = \kappa(P, T) = \kappa(\phi),$$

$$\tau = -\int \kappa(\phi) d\phi / g$$

Therefore τ varies on equipotential surfaces like g^{-1} . Solution of the transfer equations yields the temperature which is not constant on equipotential surfaces. This can be most easily seen in the case of a grey atmosphere where radiative equilibrium in the simplest approximation is given by

$$T^4 = \text{const.} \times T_{\text{eff}}^4 \left(\tau + \frac{2}{3} \right).$$

T_{eff}^4 varies on equipotential surfaces like g and τ like g^{-1} . Therefore T is not constant on equipotential surfaces. This is in contradiction to the condition of hydrostatic equilibrium. The equilibrium condition with the longer time scale will not be fulfilled. This is the equation of hydrostatic equilibrium. We therefore must assume that there are strong horizontal motions with velocities high enough that the inertia terms are of the same

order as the pressure gradient. This means the velocities are near the velocity of sound.

The theory of atmospheres of rotating stars has recently been worked out by C. Smith (1970) and indeed he found that there are velocities which come near the velocity of sound. Therefore if chromospheric activity is found in rapidly rotating hot stars where convection cannot account for it, turbulent atmospheric motions in the atmospheres caused by rotation may be responsible.

ACKNOWLEDGEMENT

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DISCUSSION FOLLOWING THE INTRODUCTORY TALK BY KIPPENHAHN

Skumanich — This may be quibbling with numbers, but if you use the revised age of the Hyades that Conti and von de Heuvel suggest then, in fact, I get that the rotation and calcium emission curve decay with an inverse cube root. But then the rotation, the lithium, and the calcium emission very rapidly decay past the Hyades point. Maybe that's due to the appearance of the Goldreich-Schubert strong mixing but in any respect there is this uncertainty about the ages.

Shatten — With regard to the mass loss term. I presume what you mean is the particle mass loss term which mostly affects the angular momentum, to distinguish that, as we said before, from the mass loss term of the star itself, mostly due to the loss of photons.

Kippenhahn — It takes 10^{11} years to get a loss of mass from the Sun due to the mass equivalent of the radiated energy. Correspondingly, the loss of angular momentum is negligible.

Jennings — I'd like to point out that on the pentagon diagram you had the coronal heating directly connected to mass loss. I think Weymann has shown that for late type giants and supergiants, there seem to be rather serious observational problems with that particular mechanism.

Kippenhahn — The arrows in my diagram just indicate possible influences they do not necessarily indicate important effects. The arrow in my diagram which indicates the influence of mass loss from coronae on stellar evolution presumably does not indicate an important effect, either.

Jennings — There is one other point I'd like to make. It seems possible that grains may drive mass loss. If that is indeed the case, and one has a grain field around some stars it would act as a strong sink for heating. One would have inelastic gas-grain collisions and the grains would radiate away a lot of the energy that might normally be deposited in a chromosphere-corona.

Skumanich — This again doesn't change your results. But I might say that Durney's argument follows even without assuming the mass loss, M , to be a constant. In fact one can show that the product of the mass loss times the Alfvénic "gyration" radius squared goes as B^2 . So maybe we should look for that other little square root in the moment of inertia; maybe the revision of the Hyades age is correct.

Kippenhahn — One can repeat Dr. Durney's computations with different assumptions. But one always gets something similar to the Skumanich law.

Underhill — I'm concerned about the remark you made, that towards their later stages of evolution stars, on their outside, don't really know what their age is inside. This rather worries me, because we stellar spectroscopists look at the outside of stars and say that's part of the star, therefore the star must have such an age.

Kippenhahn — If a star of a given mass comes twice during its lifetime to the same point of the HRD its spectrum should be the same unless chemically more evolved material has been brought from the deep interior to the surface. This is not in controversy with the usual age criteria, which either compare stars with different original metal content or different positions on the HRD. If a star, in its later evolution, happens to cross the main sequence, it normally will have a slightly higher luminosity than it had during its first main sequence stage. But, in principle, it would be difficult to distinguish whether the star is a real main sequence star or just an occasional visitor.

Underhill — That's what worries me, because every time we see a star of a certain type, we go to the first possibility and ignore the second.

Kippenhahn — What I said only holds for simple stellar models, corresponding to the outer ring of my pentagon diagram. But this is insufficient. The boxes of the inner ring are important too. They involve rotation and magnetic fields. The star coming to the main sequence for a second time would differ in its rotational properties. Therefore the Skumanich law should help you to distinguish between a young star and an old star at the same point on the HRD. There is another point which I would like to comment on. In the picture I sketched in my talk, a star like the Sun would slow down its rotation on the main sequence and, after a while, the dynamo would be rather weak and the enhancement of mechanical flux by magnetic fields would be small; the Ca emission would be weak. When the star leaves the main sequence and moves into the red giant area of the HRD, its angular velocity is getting even smaller, due to conservation of angular momentum. But at the same time convection becomes more violent. So we have two effects acting against each other: Rotation which goes down and convection which goes up. Which will win? But it would be possible, also, that even with slow rotation the dynamo becomes more active, since it has not yet been investigated how the effectiveness of the dynamo changes, when convection becomes stronger while rotation becomes slower. We also do not know how the enhancement of convection will affect the differential rotation. We therefore are unable to predict whether the Ca emission of the Sun will come back when the Sun will become a giant star.

Pecker — My comment is related to the question by Anne Underhill. Of course, the question she asked is: Are we right to use a 2 dimensional

diagram to represent stellar spectra. And the reply is of course "no, we aren't right." To come back to the specific question: "Does the K line emission enable us to distinguish between the pre-main sequence or the post-main sequence stage?", I would like to refer to a computation which has been made in Nice by Nicole Berruyer. She shows (using Larson-Starrfield kind of techniques), that, when you reach the main sequence for stars of high mass (~ 20 solar masses), then the time of contraction of the envelope is long compared to the time during which the star is staying on the main sequence before leaving the main sequence. Therefore, near the main sequence, you cannot distinguish easily between pre and post main sequence stages; both stars still have very large envelopes. At the opposite, for a lot of stars in the H-R diagram (in the pre-M dwarfs for example, where you have the T, Tauri stars) it is exceptional to find an example which is still in contraction, because the lifetime on the main sequence is 10^{10} yrs. vs 10^5 - 10^6 yrs. for contraction of the envelope. I think we should certainly look at things in the spectrum that are oriberia of the age of the stars, and others that are oriberia of the age of the envelope.

Aller — We can look at the problem of solar and stellar chromospheres from several different points of view. One point of view, which was emphasized yesterday, is understanding the manner in which chromospheres are created and heated in the neighborhoods of stars. At the outset we assume that chromospheres exist. Furthermore, we have some biased view of what they ought to be like from observations of the solar chromosphere. Now, how can we make use of this information in investigating the radiation of other stars? Here, of course, we are severely limited by the nature of our observational material. Whereas we can make detailed observations of the structure of spicules and other fine points of the solar chromosphere, observations of stars involve only their integrated light. It is true that one can make time resolved studies. These have shown, for example, rapid spectral changes in the emission lines of some stellar envelopes. Whether you call them chromospheres or not depends on your point of view. My favorite star in this respect is HD 45166 whose rapid variations were discovered many years ago by Carol Anger Rieke at Harvard Observatory. This is perhaps an extreme example. The question before the house is to what extent can we make use of chromospheres to evaluate the status of a star with respect to its evolutionary development. This was the point which was raised by Anne Underhill, and it is a matter which concerns many observers. For the most part, we are limited now to a narrow spectral range. Part of the material we urgently need falls in the "vacuum" ultraviolet and, until we get a proper space telescope, we are going to be frustrated in our efforts to get even a rough picture. In the meantime, we have to get by with

what we have. In addition to conventional spectroscopic observations, we also have some radio data for a few interesting binary systems, though we have not yet begun to understand the physical significance of what we are observing. The rapid rise in the efficiency and sophistication of infrared techniques will undoubtedly give us a great deal of information about this important spectral region. This infrared radiation may not *all* come from dust clouds, as is the favorite hypothesis today, but some of it may come from bona fide chromospheric activity. Therefore, from the observational point of view, there are only a very small number of handles that we can grasp, a very small number of things that we can do. Those of us who are observers would like to have the help of theoreticians who may point out what are the specific observable phenomena that we should seek in different stars in different parts of the Hertzsprung-Russell diagram in order to get clues as to evolutionary development.

Durney — I would like to discuss some work I have done recently with John Leibacher of JILA on the location in the HRD of different types of stellar winds. In a recent paper, Roberts and Soward (1972) have determined in the N_0 , T_0 plane (the density and temperature at the base of the corona) the regions where the stellar winds are A) supersonic for distances larger than the critical point located outside the surface of the star (usual stellar winds), B) always subsonic (stellar breezes), and C) supersonic for all distances larger than the surface of the star. With the help of Kuperus' (1965) calculations of N_0 and T_0 for a variety of stars, we locate the stellar winds of type A), B), and C) in the Hertzsprung-Russell diagram. The relevance of static envelope models for stars with winds of type C) is discussed.

Figure IV-7 is taken from Roberts and Soward's paper (1972). In the following, we designate by "subsonic" the "Chamberlain" region of Roberts and Soward. This is not quite proper, and we refer to the above paper for a more detailed discussion of this region. If N_0 and T_0 are located to the right of the dashed curve, then the stellar wind is supersonic from the surface of the star outwards. Since T_0 is given in units of $\frac{GMm}{kR_0}$ (where G is the gravitational constant, m half the hydrogen mass, M and R_0 the mass and radius of the star and k the Boltzmann constant), it is clear that if R_0 is large and the star has a corona with a typical coronal temperature and density, then the stellar wind, according to Figure IV-1, will be supersonic for all distances larger than R_0 . As an example, if we assume $T_0 = 2 \times 10^6$ K and $M = M_\odot$ the critical radius of the star for which the stellar wind is supersonic from the surface outwards is $\sim 13 R_\odot$ for $N_0 > 2 \times 10^5 \text{ cm}^{-3}$.

We discuss now the physical meaning of stellar winds which are supersonic at the surface of the star. It is well known that the stellar wind

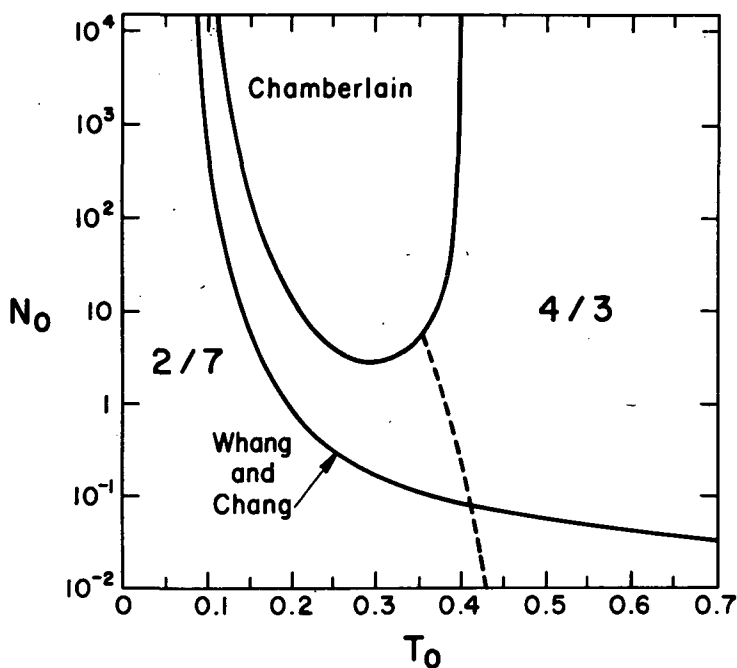


Figure IV-7 The types of acceptable solutions of the stellar wind equations as a function of the temperature, T_0 and density, N_0 at the base of the corona (T_0 is measured in units of GMm/kR_0 and N_0 in units of $2\kappa_0 (\text{GMR}_0)^{-1/2}/k$ where $\kappa = \kappa_0 (T/T_0)^{5/2}$ is the electron conductivity). In the regions denoted by $2/7$ and $4/3$ the asymptotic behavior of the temperature is $T \sim r^{2/7}$ and $T \sim r^{4/3}$ respectively whereas $T \sim r^{2/5}$ for the Whang and Chang line (c.f. Durney and Roberts 1971). To the right of the dotted line the flow is supersonic at the surface of the star. (From Roberts and Soward 1972).

equations allow for two degree of freedom: it is possible to give arbitrarily N_0 and T_0 or alternatively the mass flux, C , and the residual energy per particle at infinity, ϵ_∞ . The mass flux, C , is introduced in the momentum equation by the use of the continuity equation, and ϵ_∞ is the arbitrary constant appearing in the integral of the energy equation. These two equations are of first order and the two boundary conditions which determine the flow speed and temperature are (a) $T \rightarrow 0$ as $r \rightarrow \infty$ and (b) $p \rightarrow 0$ as $r \rightarrow \infty$, i.e. the solution should cross the critical point. This last boundary condition disappears when the stellar wind is supersonic from the surface of the star and the problem becomes undetermined. The mass flux, for example, could be given, between limits, arbitrarily. In such a star the solution of the stellar wind equations is not as simple as it is for the Sun. The heating of the corona by acoustic waves must be included explicitly and the equations must be started from the chromosphere where the velocities are subsonic. Static envelope models for these

stars are probably not meaningful. Ulmschneider (1967) has calculated the structure of the outer atmosphere of cool stars. By virtue of the above, however, we consider that his determination of the initial flow Mach number, M_0 , is very approximate; M_0 should be determined by requiring only that $T \rightarrow 0$ as $r \rightarrow 0$. The supersonic or subsonic character of the flow cannot be prescribed as a boundary condition.

With the help of Figure IV-7, and values of N_0 , T_0 and R_0 as evaluated by Kuperus for a variety of stars, it is possible to give the approximate location of stellar winds of type A), B), and C) in the Hertzsprung-Russell diagram. This has been done in Figures IV-8 and IV-9. There is no doubt

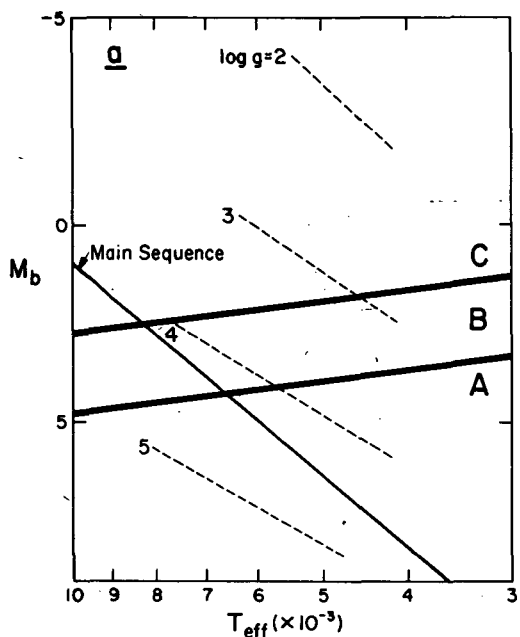


Figure IV-8 The mass of the star follows the mass luminosity relation. Regions A), B), and C) have been determined from Figure IV-1 and from the values of N_0 , T_0 and R_0 as evaluated by Kaperus (c.f. Figure 22 of Kaperus (1965)). Region: A) usual stellar winds; B) stellar breezes; C) the flow is supersonic: at the surface of the star.

that Kaperus calculations are very approximate. However, since in classifying stellar winds according to type A), B), and C) the values of N_0 and T_0 are not too critical we can have some faith in the general location of regions A), B) and C) in the Hertzsprung-Russell diagram. We stated above that the validity of static envelope models of stars in region C) had to be carefully examined. It is tempting to speculate that stars with very large

radii can suffer appreciable mass loss by this process of "coronal evaporation" (c.f., Weymann (1960), for the case of red giants). The importance of radiation pressure in the mass loss of hot stars and stars with circumstellar dust shells has been considered by Lucy and Solomon (1969), and Gehrz and Woolf (1971). Further work on this subject is in progress.

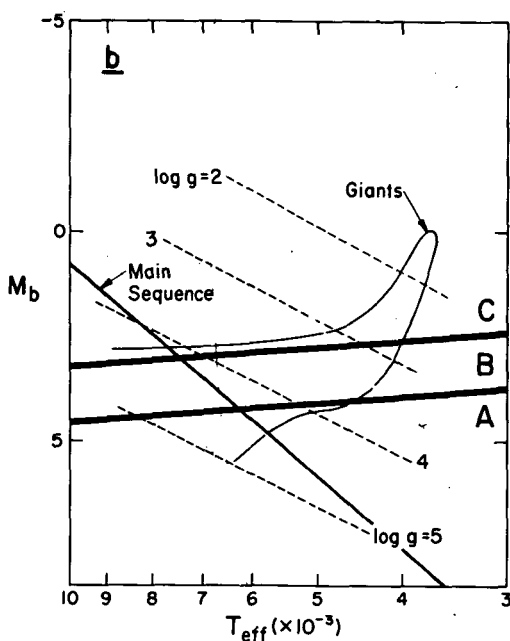


Figure IV-9 The mass of the star is equal to the mass of the Sun. Regions A), B), and C) as in Figure IV-2a).

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DISCUSSION FOLLOWING TALKS BY PRADERIE AND DOHERTY

Heap — We can see where the velocities are perhaps supersonic at the surface of the star. What do you mean by the surface? Is it the stellar evolutionists' surface or the photosphere?

Durney — T_0 is measured in units of GmM/kR_0 and N_0 in units of $2\kappa_0(GMR_0)^{-1/2}/k$; R_0 is the surface. In general R_0 would be the distance at which energy deposition takes place.

Heap — My next comment is on the stars populating your Region C. A regular O star is in Region C, and a planetary nucleus would also be in Region C. Observations of these stars tend to support your suggestion that Region C objects have some sort of chromosphere. As I mentioned earlier, both types of stars show a velocity-broadening that is 75 km/sec or greater. In the case of some Of stars, both young stars and the very old planetary nuclei, the HeII $\lambda 4686$ line is very broad, indicating velocities up to ± 1000 km/sec, so these stars have a mechanical flux which could possibly be dissipated in forming a chromosphere. There is one young Of star, Zeta Puppis, showing broad HeII $\lambda 4686$ emission whose chromosphere in fact has been seen. The UV spectrum of this star has been observed from rockets by Morton and Smith, and it shows several high-excitation emission lines. These UV lines would be an indication of a chromosphere, because the excitation of, say the O VI emission line, certainly is greater than that of the photosphere. No planetary nucleus has been observed in the rocket-UV, but there is possible evidence for chromospheric enhancement of radiation in the far-UV, below the HeII limit at 228Å. The evidence lies in the discrepancy between the HeII Zanstra temperature and the temperature derived from the visible spectrum of the star. For example, the nucleus of NGC 2392 has a HeII Zanstra temperature of 94,000°, while the visible stellar spectrum indicates a spectral type of O6 or O7. Perhaps the nebula is "seeing" chromospheric radiation from the resonance lines of high-ionization states of C, N, and O rather than photopheric radiation.

Durney — You are right, the temperature range is too large. This is because the figures shown are identical to Figure 22 of Kuperus. The division of the Hertzsprung-Russell diagram into regions A), B), and C) applies only to those stars for which Kuperus did evaluate N_0 and T_0 . In particular he did not calculate N_0 and T_0 in the high temperature region of the figures.

Jennings — When you say that this type of flow might affect stellar structure, is that to mean that the quasistatic approximations for calculating models would not be valid?

Durney — For calculating envelopes they probably break down.

Jennings — I would argue by continuity that the interior would not know anything about this mass loss.

Durney — In general, I think it would not.

Cassinelli — I think it should be pointed out that radiation pressure effects become important in region C. At the higher luminosities the outward acceleration due to the radiation pressure gradient may be greater than, or equal to, the inward acceleration of gravity.

Durney — Right. In this calculation the radiation pressure was not included.

Ulrich — How sensitive is the location of the boundaries on the H-R diagram to the mechanism you used to derive those values. You don't know what the coronal temperatures are.

Durney — We have accepted Kaperus results. From Figure IV-7 and the units in which T_0 is measured (GmM/kR_0) we expect regions A) and C) not to change much for values of T_0 not drastically different from typical coronal temperatures. This is because the range of variations in mass is smaller than variations in radius. Region B) demands a more sensitive balance of N_0 , T_0 , and R_0 , and could, for example, disappear if Kaperus calculations are seriously in error.

Aller — So evidently in these hotter stars the wind starts blowing very close to the stellar surface so that you do not have a conventional chromosphere. I mean you don't have a semi-steady one like the solar chromosphere; the wind is blowing all the time; the material is always flowing outward. Is that your conclusion?

Durney — Yes.

Underhill — But that doesn't mean it's not a chromosphere. We didn't define a chromosphere as having to stay still.

Aller — The physicist's problem is that one must consider differently a mass of gas which is moving violently outward from one which is quasisteady. The velocities are already large at the surface of the star.

Thomas — If I were to paraphrase what Delache said yesterday, what you just said is not true. All that happens is, when I look at a sequence of stars, maybe I have to worry more and more about the outward component of the mass flux as I change the spectral type. Sure, I agree in detail it's different, but, in terms of the broad physical picture, it is not.

Aller — Precisely these details may be very important in an interpretation of the data and analyzing the obtainable observations which may admit several, equally plausible, but different pictures.

Thomas — The details are always important, but unless I know the structure first, I become a number juggler. You must first have a structure; then of course, if *all* you have is a structure, you're going to miss the details. I must have the detailed computation of the numbers to be able to convert the observations. But if I just compare the numbers without having the structure, then it looks as though each different star is a problem by itself; and that is a viewpoint I disagree with.

Aller — I would disagree with that too.

Kippenhahn — Am I to understand that you want to redo the work and replace the normal static by kinetic boundary conditions? One would then expect that this would remove the difficulty that near the surface you get supersonic velocities. Then everything would look rather decent. The difficulty that you encounter is that you use stellar models with static atmospheres and fit to these dynamic atmospheres.

Durney — This proves that one cannot construct static models. One needs to construct consistent kinetic models.

Pasachoff — Again here we have to be careful that the definition that we get for a chromosphere does not exclude the solar chromosphere, which is not at all static. The solar chromosphere is probably composed entirely of spicules, which have velocities of approximately 20 km/sec.

Durney — But the outward flow velocity is small in the solar chromosphere. It is only randomly non-static.

Underhill — I think we're just hung up on the fact that an observation of a line profile gives you an average over the stellar surface. It can frequently be a net general outward velocity. What you're saying about spicules in the Sun is that you're looking at individual features and you can see there are large changes. It is a question of statistical averages.

Aller — That is correct. The fine structure doesn't change things very greatly. I think there is a rather important qualitative difference between a chromosphere like that of the Sun and the atmosphere of a P Cygni star. Aside from the greatly different temperatures involved, the inherent differences in the velocity fields give them a very different character. I'm not saying they shouldn't be called chromospheres, but I think we have to be aware that the definition may embrace envelopes that are almost, but not quite, in hydrostatic equilibrium, on the one hand, and also evanescent structures, where you have a violent wind blowing, on the other.

Stein — In looking at your first figure, one might turn that around and say that the boundary of the region where the flow is supersonic from the surface, gives an indication of the maximum temperature which is possible in a corona, since, for higher temperatures, such a large mass loss (and therefore energy loss) occurs, that the temperature is reduced to approximately the critical value. This might provide a limit to coronal temperatures.

Durney — Yes, but there is some arbitrariness in this. We think that one needs to solve the problem consistently.

Stein — It's true that it's arbitrary, but because the thermal velocity is approaching the escape velocity, the maximum possible corona temperature must be of the order of your T_0 .

Durney — I agree.

Skumanich — I'm disappointed that Dean Petersen hasn't said anything. I received recently a preprint from him about the problem of a radiation driven flow in which the sonic point is inside the envelope. He uses a plane parallel approximation to the flow, because he's dealing with a fairly large radius, but I think the physics is really the same. You only get a C^2/r term difference in the driving forces what he finds is that the flow is decelerating after it goes through the sonic point. It reaches a maximum and then becomes a decelerating flow. But what bothers me about the work is that there is a finite pressure, a wall as Delache said the other day. I don't know where it comes from and what its consequences are on the actual detailed dynamical flow. Is this wall the back pressure of the outward traveling shock that the wind has ultimately produced in the interaction with the interstellar medium? There must be some time dependent phenomenon at the leading edge of this wind and at the tail where the rarefaction is eating into the envelope of the star. So these steady flows are quasisteady flows in the sense that they settle down to a constant form in space, but they have time dependent leading edges. I don't know what that does to this whole problem, the time variations and so on.

Pasachoff — Though we haven't agreed on what a chromosphere is in the general case, I thought that we should at least show the meeting a picture of one, so that we know what it really looks like. Figure IV-10 is a photograph taken of the solar chromosphere in H α at the Big Bear Solar Observatory on May 22, 1970. It represents the current state of the observer's art. Resolution is better than 1 second of arc.

Underhill — I'm not at all sure about the revisions of T_{eff} vs B-V around type AO V. The revision you were talking about brought Vega down from 10,000 to 9750°K.



Figure IV-10

Conti -- No. It was 10800 to 9750°K.

Underhill -- So it brought T_{eff} down 1000°K. Consider the problem of the large ultraviolet blanketing which I discussed a couple of days ago. If you enter that into our standard model calculations, you get the usual effect of backwarming. The net result is, as Deane Peterson mentioned, you can probably make a fully line blanketed model for Vega that fits all of the visible region with an effective temperature of the order of 9300°K. This is one of the difficult things that you have to remember about model atmospheres: they are only models, and every time we put some more factors in them, this parameter, T_{eff} , which is essential for stellar interiors comes out a bit different. The real problem is that this parameter is not at all essential for stellar atmospheres. We're trying to tie this B-V to a non-essential parameter. In fact stars can have exactly the same spectrum and be of entirely different ages. You have to realize that this pillar of stellar structure is no pillar whatsoever for the stellar atmosphere. You have to look for another pillar.

Conti -- There was some discussion by Andy Skumanich on the revised age of the Hyades from a paper by van den Heuvel and the applicability of his number. I should say you better believe it. If you look at his paper, which is in the P.A.S.P. of about 2 years ago, you'll see that when you draw a theoretical H-R diagram with the turn-off age between 8 and 9×10^9 years, for the Hyades, it matches extremely well all of the members of the cluster. That's where the age determination comes from. The reason that the age was revised by about a factor of 2 was that when you go from a theoretical H-R diagram to an observed H-R diagram, you've got to make some connection between a B-V color and an effective temperature. What had happened was that the T_{eff} vs. B-V relationship for A type stars had been altered, primarily because of the continually changing temperature calibration of Vega. It has now pretty much settled down, and what van den Heuvel realized was that this would effect the turn-off diagrams and, therefore, the evolutionary times of clusters that have turn-off points somewhere in the A stars. For example, this does not affect the Pleiades nor M67 but it does affect the Hyades. This somewhat more elderly age for the Hyades does now have implications for the solar system, the lithium depletion, and the H and K emission, and so on, as Andy mentioned. That's the first comment. My other comment has to do with massive stars. We've heard that the star forms and that the star starts burning nuclear fuel, and that the envelope still doesn't have enough time to fully contract. This is the work of Larsen and-Starrfield. I think this has direct application to the star I discussed on Tuesday, θ^1 Orionis C, where we see material accreting. I just wanted to make that connection.

Pecker — I want to reply to Peter Conti. Consider the H-R diagram. Now we've just discussed the calibration of T_{eff} . I want to draw attention to one thing which is extremely relevant to the problem. In those stars with strong emission lines, very often infrared excesses are strong, but not always known. Then bolometric corrections are absolutely wrong. For example HD 45677, a Be star, has a bolometric correction a little bit more than one magnitude in error, compared to the value given by the classical B models. That is my first comment.

The second comment is linked with what Peter Conti said about the reversed P Cygni profile observed in a Trapezium O star. Such P Cygni profiles are associated with contracting (or pre-main sequence) objects. There is another case which no one has mentioned yet at this meeting, and that is FU Orionis. I would like to draw your attention to a series of papers which has been published in Russian by Ambartsumian (and so far translated for me by an Armenian astronomer). This comment is quite relevant to the origin of the heating. Ambartsumian is regarding the Hayashi like theories for contraction before pre-main sequence stars as unsuitable for objects such as FU Orionis. He is assuming that there is a new class of objects that he calls "FUORs", the first one of them being P Cygni itself. I might recall the fact that P Cygni is now of a magnitude which is visible, while at the time of Tycho Brahe it wasn't. This is the reason which makes Ambartsumian think it is a star of this type. P. Cygni is number 1, FU Ori is number 2, in this series of objects. Number 3 is Lick Ha 190. According to Ambartsumian, a FUOR is a superdense star, a member of a binary, and from time to time the super dense star is throwing away high energy particles, which are heating the outer part of the other star. This is what creates the chromosphere and its abnormal heating. I just wanted to draw this to your attention because I don't think we've been exploring all the possibilities of heating. We have so far been trying to concentrate only on the heating from inside. My question is, are there any possibilities of heating *from outside*?

Then I come to my third point, which is a question for Dr. Kappenhahn. (Now let's forget about this reference to Ambartsumian; I don't know whether or not I can believe it. I think I am myself more in favor of the classical contraction theory of Larson, or Penston, especially for the interpretation of FU Orionis.) My question is: when you have a pre-main sequence star, in pre-main sequence evolution, then you have something which contracts. To avoid confusion, let's not take a hot star where there is the extension of the HII region which mixes up the problem. Let's take a cold star. There is some energy which is released by contraction of the mass. Now where is this energy liberated, what is the quantity of energy which is liberated, and can it contribute to the formation of a chromo-

spheric heating and of a chromosphere? I ask this question specifically for the T tauri stars.

Kippenhahn — Do you have in mind that the star is contracting and probably there are some outer layers which follow more slowly? We come back to the old problem of meteorites falling on top of a star and heating the outer layers. I would say this is still possible for the T Tauri stars, although what we observe is that there is mass loss from these stars and no infalling material. But, on the other hand, if you look at the Larsen solutions of the problem of star formation, you find dust clouds raining on top of stars for long periods. Material is falling on stars which have just been formed. They might already be close to the main sequence. What will happen with the kinetic energy of the infalling material? Certainly this is a problem which should be looked into.

Kuhi — I think bringing these stars into the discussion is going to throw the field wide open for drastic speculation. It is interesting to me that the calculations by Larsen and others for contracting stars always show material falling in during the contraction phase, and also a large amount of dust surrounding the star which presumably then reradiates in the infrared. Aside from about six or seven stars in Orion, there is no evidence for any infalling material in any contracting star that I am aware of. It is ironic indeed that Peter Conti should mention a star which is a very high temperature object, with which we normally associate a large HII region, a large radiation pressure, and from which we would normally think material is being driven off the surface. On the other hand, he finds material falling in. It seems to me that somewhere our theory is in drastic error. The point about the dust clouds surrounding these young stars is a very good one. I should mention the observations of Gary Grasdalen (a graduate student at Berkeley) of stars like Lk Ha 190, which is also known as V1057 Cygni. This object was an extreme case of a T Tauri star before it blew up (or whatever else it did), having a very rich emission line spectrum which some of us would call a chromosphere. Anyway, if we accept Larsen's picture, then we must also accept a large infrared contribution to the flux for this object in its pre-outburst phase. After its outburst it was indeed a bright infrared object, so we might say everything is fine. However, Grasdalen has looked at a number of T Tauri stars in the same part of the sky which have virtually identical spectra to the pre-outburst Lk Ha 190 spectrum, and he finds no infrared excess whatsoever. So I think that our theoreticians have much further to go than they would be willing to admit.

There is one other point that I would like to add about bolometric corrections. The infrared observations have cast considerable doubt on our old ideas concerning even the hot stars. Many of the hot stars, especially

Ae and Be stars, have shown large infrared excesses, and when one adds these to the total fluxes emitted by the stars to get the total bolometric luminosity, I think we find again serious discrepancies with previously held ideas. The same thing applies to the pre-main sequence contracting stars. You often find that the luminosities in the infrared are many times that in the visual and coming back to the T Tauri stars, you find that you need masses much larger than the previously assumed one or two solar masses to explain the total luminosity. Just to make one concluding remark, Lick H α 190, a typical T Tauri which we all thought was one solar mass popped up and is now an A supergiant. Explain that.

Aller — You're giving the theoreticians a pretty rough boundary condition.

Böhm — I would like to ask Peterson: how can you calculate mass loss in a plane parallel approximation? Isn't it true that in a plane parallel approximation you have to do an infinite amount of work to push matter to infinity? I don't see how one can ever get mass loss, but maybe I misunderstood something.

Peterson — That's right. Because it is an artificial geometry, you have to impose a sink at the top of the atmosphere. Basically, it shows up in the equations as a finite boundary pressure. Fortunately the equations do not leave that boundary pressure a free parameter.

Böhm — So this pressure which was mentioned is somewhat artificial.

Peterson — It's artificial, yes, and it goes away in the spherical case.

Lesh — I'd just like to add something to Anne's comment that stars can have the same spectrum and still have widely or slightly different ages. We have been looking at a class of B type variable stars, the β Cephei stars. As a star evolves away from the main sequence it turns around at a certain point and describes a loop in the H-R diagram, as you well know. Near the turnaround point, a star can actually be doing quite a number of things. It can be evolving away from the main sequence; it can be contracting back; it can be burning hydrogen in a shell source; and, in addition of course, it might be contracting towards the main sequence. In a particular small region of the HRD, there are a large number of normal non-variable B stars, but there are also about 20 of these odd creatures called β Cephei variables. It seems very likely that they (variable and non-variable stars) occupy the same region of the H-R diagram, because they are in different stages of evolution, in other words, because they have slightly different ages. However, the work I have done on these stars with Morris Aizenman at Montreal has shown that there doesn't seem to be any spectroscopic distinction between the variable stars and the

non-variable stars. So it would appear that here we have a case of stars which do, in fact, have slightly different ages, if we assume that contraction towards the main sequence is ruled out, but which do not differ in any observable spectroscopic fashion.

Aller — This would appear to be another incidence where the surface of the star doesn't pay any attention to what the interior is doing.

Boesgaard — I'd like to discuss α Centauri in connection with differences in the chromosphere with stellar age. α Centauri is a triple system. The first component A is exactly the Sun observed at stellar distances; it's a G2 V star. Component B is a K1 dwarf, and component C, Proxima, is a dMe flare star. The fact that component C is a flare star would indicate that it, at least, and presumably all three stars, are probably young. However, the intensity of the chromospheric calcium emission in α Cen A and B, gives, as far as I can see, no indication that those two stars are young. α Centauri A has very weak calcium emission. It looks similar to the Sun. At 3.3 Å/mm on a long exposure one can just see weak K2 features. I have two long exposures of this taken at Mauna Kea. (We can get down to declinations of -60° .) The two spectrograms look slightly different in the K2 structure. In one case it looks like the red peak is stronger than the blue; in the other case it looks like the 2 peaks are of equal intensity, but I'm not willing to say that this represents a solar cycle type of variation, or the kind of local variation you see in the Sun, because the emission is so weak.

The K1 star shows a calcium intensity of 2 on the Wilson scale. This was also observed by Warner. That's about the relative intensity you'd expect, for the relative temperatures of the two main-sequence stars.

Aller — Do we really know enough about emission processes in dMe stars to apply this rule? I was under the impression that α Centauri C was a fairly "late" M dwarf, that is to say, advanced in the sense of spectral type, in other words, a very cool object. I wonder how well the calibration works down in that spectral region.

Mullan — There is unfortunately no simple relationship between the age of a flare star and its level of flare activity. Haro and Chavira (Vistas in Astronomy 8, 89, 1965) observed flare stars in seven clusters ranging in age from the Orion group to the Hyades. They found that, as a flare star evolves towards the main sequence, it flares more frequently. This was directly opposite to a prediction of Poveda who believed that the youngest flare stars high above the main sequence should have retained fossil magnetic fields, and should be more active than older flare stars near the main sequence. However, observational selection could account for the effect discovered by Haro and Chavira if the absolute luminosity function of flares is the same in stars of different type.

On the subject of fossil magnetic fields, I would like to supplement what Professor Kippenhahn said about dynamo fields by pointing out that fossil fields may also be important in understanding stellar chromospheres. Unsöld showed that the decay of emission in the H and K lines of Ca II can be understood in terms of the decay of fossil fields by Joule dissipation. This is not to say that dynamo fields are never important. For example, although flare stars, in all likelihood, require strong surface magnetic fields, it may not be important whether the fields are fossils or have been generated by dynamo action. A flare star might conceivably go through two phases of flare activity, one in which its field is fossil, the second in which the field is dynamo generated. This would help to interpret the lack of a unique relationship between the age of a flare star and its level of activity.

Boesgaard — Isn't there information on the statistics of the galactic orbits, to get an age indicator for the flare stars?

Mullan — Galactic orbits provide information about ages of field stars. The results of Haro and Chavira are confined strictly to cluster stars. In the case of the field stars, the most significant feature of the galactic orbits of M stars is Delhaye's discovery that the dispersion of peculiar velocities of dMe stars is significantly smaller than that of dM stars without emission. As a subgroup of the dMe stars, flare stars are then expected to be, on the whole, a young group. But within a group so young, age discriminators are not really available.

Boesgaard — Any connection between that and the amount of flare activity?

Mullan — I don't know.

O. Wilson — About a year ago, Woolley and I had a paper in the Monthly Notices in which we compared the results that I got on about 400 (Vissotsky) stars, on which I made very careful eye estimates of the intensity, with the predictions of galactic dynamics, which are that the older the group of stars, the greater should be the eccentricities of the galactic orbits and the greater the inclinations. This correlation was extremely good. There were no flare stars in the group, or there were so few that they didn't matter. But just looking at the spectra, I would say that the flare stars form a continuation at the end of the sequence where the calcium emission is very strong and where you see Balmer emission; they lie just a little bit farther along. But of course they're relatively rare.

Aller — And you would conclude that these are relatively young stars.

O. Wilson — I think there's no question about it.

Underhill -- But the question is: does young mean a fraction of the total evolution track, or does it mean literally counted off in seconds as determined by atoms on the earth?

Aller -- I presume that it means young in the sense that α Centauri A and α Centauri B would not be as old as the Sun, according to this reckoning.

Boesgaard -- That's my impression, because C is a flare star. But I don't have any estimate in years.

Kippenhahn -- I would like to comment on the question about the fossil fields. In that area of stars where we deal with calcium emission, we have no evidence of fossil magnetic fields. In the case of the Sun we have a dynamo generated field. With the dynamo fields you can expect an age dependence of the chromospheric activity, as it is observed, while for the fossil fields you would have a time independent chromosphere.

Jennings -- I have a clarification question. At what dispersion were those observed?

Boesgaards— 3.3 Å/mm.

Jennings -- Have you actually traced them to see what the emission percentage is, and is it about 4% of the continuum like the sun?

Boesgaard -- Approximately. I don't have an exact number.

Kandel -- I must say that the pictures occasionally have been puzzling. Several years ago I looked at 61 Cygni B at 10Å/mm. 61 Cygni is generally said to be old, associated with a group which has an H-R diagram like M67. Yet, it has awfully big H and K emissions. I couldn't resolve whether there was a central reversal there, but the emissions themselves were rather big. I think Dr. Wilson has observed variations there. Perhaps he would comment on that.

O. Wilson -- I will talk a little bit about this subject this afternoon and, while 61 Cyg B has certainly a well marked emission, I can find you other stars of similar type that have 2 or 3 times as much. So it's a relative matter.

Aller -- I would like to ask some of the stellar evolution people if they have tried to determine an age for the α Centauri system by seeing how well it fits the general main sequence. My impression is that it has not evolved off the main sequence by any distance sufficient to allow us to draw any conclusions. That's why it will be difficult to get its age by evolutionary arguments, even though it is certainly a star whose mass, luminosity, and perhaps even radius, are very well known.

Kippenhahn — I would guess that it is not possible to do this. Just the uncertainties we have in the opacities may spoil the whole picture.

Steinitz — It has been mentioned a few times that there is a connection between the age and the characteristics of the spectrum in the atmosphere. One thing that has been mentioned is the chromospheric activity. It has been claimed that the atmosphere doesn't know about the age of the star below it; as an example, the β Cephei stars have been taken. I would like to mention the mere fact that we have classified them; that they look like other stars in the same region; yet, they oscillate while the others don't. So obviously the atmosphere knows that something else is going on.

Lesh — The fact that some of the stars oscillate while the others do not does not mean that the atmosphere of the stars knows how old they are; the interior does. It is very likely that the oscillation arises in the interior and not in the atmosphere.

Steinitz — It is an age effect.

Lesh — Yes, it is an age effect in the interior and not an age effect in the atmosphere.

Aller — I don't know to what extent we want to discuss the spectra of oscillating stars. That is a fascinating field in itself, but perhaps we'd better settle this question first.

Hack — The line contours are rather different in the spectra of normal B type stars and in the spectra of β Conis Majoris, which sometimes show one, two, or three components variable with time and having different radial velocities. So I don't agree that they are equal to the normal main sequence stars.

Aller — Well, certainly with high dispersion, the spectrum of σ Scorpii, for example, doesn't look just like that of a normal B star. There are important differences. Please tell us what dispersion you are using. We are talking about utterly different problems here in the sense that the K line effects mentioned by Mrs. Boesgaard can be detected only by going to very high dispersions, of the order of $3\text{\AA}/\text{mm}$. They are very small effects, whilst the effects that you see in some of these oscillating stars like σ Scorpi, which belongs to the β Cephei class, are fairly obvious at relatively low dispersion. The changes are probably photospheric effects rather than chromospheric effects or strictly upper atmospheric effects of some kind.

Conti — I'd like to return again to these O and Of stars, and point out that what we think is the mechanism for the emission forming region and the extended envelope has something to do with the radiation pressure. I think Cassenelli has already mentioned this. Another thing which has been

mentioned a little in the literature, but which is now receiving more attention, is the variation in the emission lines that you see in these stars. For example let's say you observe that emission lines vary on time scales somewhere between time scales of several minutes to several hours, I mean they're really drastically varying. So in addition to the extended envelope which we certainly do have, we have very good evidence of changes going on in this envelope which are very reminiscent of other kinds of stars. In fact, if I may return to my Zeta Puppians looking at the atmosphere of their star in the light of $\lambda 4686$, they might not be too surprised to see something looking a little like a solar spectroheliogram in $H\alpha$.

Aller — It would probably look even more striking than that.

Lesh — If I may just answer the comments of Dr. Hack. I'm talking about mean properties of these stars which are, in fact, observed at rather low dispersion, on the order of 60 to 100 Å/mm. It is true that the line profiles in the β Cephei stars vary, but the mean profile — unless I'm very much mistaken — is not distinct from the mean profile in a non-variable star. Likewise the colors of the β Cephei stars vary. But if you take the mean color, which is actually what you use to locate stars in the H-R diagram, it is not statistically different among the variable stars than among the non-variable stars.

Aller — That's an interesting point. I don't think it's a statement that can be made for Cepheids. Maybe Mrs. Gaposhkin could answer that. Does the mean spectrum of a Cepheid look like any other star, or can you tell it immediately from the appearance of the spectrum.

C. Payne-Gaposhkin — You certainly can tell.

Heap — What happens to the CaII emission of say, a G star and a B star when they enter the red-giant branch? What are the time-scales involved in the development of their chromospheres? If the magnetic field and calcium emission of G stars decrease with time, why do red-giants of one solar mass have strong chromospheres?

Kandel — Nobody knows, but, in principle, the calcium emission should be detectable.

Kippenhahn — The effect of rotation on the Ca lines, via magnetic fields, during the evolution, will become less and less important while convection will become more effective when the star becomes a red giant.

Thomas — We're presumably worrying about chromospheres, and I read this very ambitious statement: "what properties of stellar chromospheres vary with stellar mass and age". So long as one talks about chromospheres

associated only with the convection zone, then you're limiting your sights very much. I agree, from the standpoint of rotation, that in the example which you have given, you've tried very hard to tie in with something else, which one knows can produce a mechanical flux. Still, from my own standpoint, as one who believes that *all* stars have chromospheres, so long as you restrict yourself to only those two viewpoints, then you're still restricting your sights very much. I prefer Len Kuhi's comment of just a little while ago, that maybe the theoreticians should be more ambitious than they are. He's trying very hard to understand what is meant by a chromosphere from the standpoint of understanding. Is it indeed something which is a property of all the stars? So I think one of the things we have overlooked badly in this conference is to ask all those kinds of physical processes which can produce, in any way, any kind of mechanical flux of energy. That's why I personally like to associate the definition of a chromosphere with a mechanical flux of energy. But let's not argue. Let's take whatever definition we want, but realize that we are talking about *general* structures of stellar atmospheres.

Cayrel — I have a question related to Dr. Kippenhahn's talk. Dr. Kippenhahn pointed out that rotation, age, and magnetic fields are three related things. I remember that at the time it was said that micro-turbulence could be also correlated with these three things. The problem is that I don't really see the mechanism by which microturbulence could be related to these things. Would you comment?

Kippenhahn — I am not prepared to say anything at the moment to your question, but since I am already standing I would like to make a comment. I agree with what Dr. Thomas said. I think we should ask what are the observational facts, or how can we find out whether the chromospheres are related to convection or not. Before the meeting, when I still was very naive, I thought that the calcium emission we see in G stars indicated chromospheres. Now, I learn that if we do not see Ca emission, this does not tell us anything. We have to determine whether the lines are collision dominated or photoelectrically dominated, and — as far as I have understood the complicated story — we then still do not know whether there is a chromosphere or not. On the other hand, we learned from Dr. Praderie that the border line in the HRD between stars with Ca emission and those without is a straight line which coincides roughly with the Cepheid strip and its extension to the lower left. It happens that this line is close and parallel to the line which separates the stars with pronounced outer convective zones from those without. *Is this accidental?* Can we learn from the experts of line formation whether, from this fact, we can conclude that chromospheric activity is driven by convection? Or must we say Praderie's border line of Ca emission is just a

border line for the significance of the Ca emission as an indicator for chromospheric activity?

Thomas — My comment was not that calcium emission may not be a strong indicator of chromospheres, where it occurs, but that there are also many other kinds of indicators of chromospheres in regions where we don't find convection zones. I don't disagree with what you say. I say only: please expand it.

Böhm-Vitense — I would like to ask a question. Is the magnetic field proportional to the velocity Ω independently of the efficiency of convection? The convection is important isn't it?

Durney — Yes. We used Leighton's model for the solar cycle. This model has some arbitrary parameters which are chosen so as to reproduce the Sun's magnetic cycle. For stars with different convection zones these parameters would be different. There is every reason to believe that again B and Ω would be proportional.

Böhm-Vitense — This means that the proportionality constant depends on the spectral type, doesn't it?

Durney — Yes. The proportionality factor between B and Ω may depend on spectral type.

Ulrich — I'd like to make a connection between today's and yesterday's discussions. I think the connection of the magnetic field to these motions is really a most intriguing aspect of the heating problem. I think in order to properly understand the heating problem, we must put in the magnetic fields. This is a real challenge to the people trying to solve the heating problem. You must be able to reproduce the hot plages over a magnetically active area. Another comment refers to the fossil magnetic fields. In some solar models which I've calculated, the decay time for the fundamental mode is 25 billion years; so the field is quite constant. However, this doesn't rule out the higher modes which have decay times of some three to five billion years, so these could give time variations in times comparable to main sequence lifetimes; however, you would have a constant term in addition. You'd have to add a variable to the constant, so it might not give you the correct behavior.

Kippenhahn — If the star is rotating rapidly, we must really include the effect of turbulence and use the total pressure. If it is only slowly rotating you can use hydrostatic equilibrium as a good approximation.

Böhm — May I just add one minor point to Kippenhahn's talk. When we talk about the Lighthill output of the convection zone we must remember that this depends strongly on the helium abundance. For

example, the numbers mentioned for white dwarfs sounded a little surprising. These white dwarfs are surely helium white dwarfs. The point is that the helium convection zone persists to very high temperatures and, if you have a very dense star, a large fraction of the energy must be carried by convection. For these high temperature objects with highly developed convection zones, you get high convective velocities, which means, in turn, a high acoustic output.

Aller — Would you say that some of the non-white dwarf helium stars might have such strong convection zones that they would be good places to look for chromospheric activity?

Böhm — It certainly is true that we expect higher acoustic outputs from helium stars than from stars of normal composition.

Böhm-Vitense — I think that for a gravity of $g = 10^4$ [Cgs] the convection extends to about 13000 degrees for helium stars rather than to about 8000 degrees as for hydrogen stars.

Evans — The decline of the RCB star, RY Sgr, in 1967 and its return to maximum in 1968-70 was studied spectroscopically by a group at the Radcliffe Observatory, Pretoria. A strong emission line spectrum (originally studied by Cecilia Payne — Gaposchkin in R Cr B and attributed by her to a chromosphere), comprising mainly lines of singly ionized metals having upper excitation potential less than 6 eV, was present early on the decline. This decayed on a time scale of ~ 22 days, compared to a time scale of ~ 5 days for the initial rate of decline in photospheric radiation. The level of excitation and the effects of self-absorption declined with time. A strong continuum short of $\lambda 4000$ was attributed to CN. At minimum light only emission lines of very low upper E.P., mainly of Ti II, were present. The lines H and K of Ca II appeared broader than the rest. Broad emission with a central absorption appeared in H and K of Ca II at times during the rise and near maximum light. These observations indicate strong chromospheric activity in a helium star.

Aller — That's somewhat cruder than the solar-model theory, but the level of excitation you describe is comparable with that observed in the Sun. So in giants and even supergiants you see that we can have densities and so-called excitation temperatures not significantly different from what we have in the Sun. This brings out a point Thomas mentioned earlier about using spectra for diagnostic purposes.

Kippenhahn — I must repeat my question: Can I conclude that when there is no calcium emission there is no chromosphere either? Or would the stellar atmospheres people say that at some point in the HR-diagram the calcium emission goes away even though the star still has a chromosphere?

Pecker — Isn't it just a matter of the pressure sensitivity of the calcium emission?

Thomas — In a Wolf-Rayet star, I certainly don't observe calcium emission. However, that doesn't mean the Wolf-Rayet star can't have a chromosphere.

Kippenhahn — I am not dealing with special objects like Wolf-Rayet stars. I am interested here in normal main sequence stars earlier than F. What is the significance of no calcium emission in these objects?

Linsky — One can do a very simple experiment to answer this question. Take a simple model to represent the quiet Sun. This model will show a slight emission core for calcium. If you decrease the opacity by a factor of two or three, the emission is gone. You'll never see it. The emission is very sensitive to the optical depth in the line. As you go up the main sequence, the chromospheric temperatures are most likely hotter, because the temperature minima will be hotter, and the calcium will be more nearly completely ionized, thus decreasing the chromospheric optical thickness in the calcium line. You'll very soon reach a point on the main sequence where the emission will not be seen at all, even though you may have a very pronounced chromosphere.

O. Wilson — I'll say something about this in my talk, but it is noteworthy that the cutoff for calcium emission is amazingly sharp. The corresponding variation in mass, radius and effective temperature across this boundary is negligible. I don't know what causes this cutoff, but I think it must be something very fundamental. This whole transition takes place in a range of $b-y$ of a couple of hundredths. It's just like you'd cut it with a knife.

Jefferies — I think the answer to Kippenhahn's question is that we have really not explored the matter enough yet. Along the lines of Linsky's comments, let me draw a line on the board and say that the gas below it represents the photosphere where the continuum is formed, and then say that the chromosphere is up here above the line with the temperature increasing outwards. Now I have a certain optical thickness in the K line as I look down through this chromosphere. If I have a temperature increase and the optical thickness is greater than about three, then I should see some K line reversal. The size of the reversal depends on the size of the temperature increase outwards, and the value of the optical thickness. If for some reason, the base of the chromosphere moves in the Sun's atmosphere, we ultimately reach a situation where we have no optical thickness left — we have run out of chromosphere, and no reversal will be seen. This is a possible situation as we go from the Sun to earlier stars on the H-R diagram. It is important to search for other

sensitive diagnostic tools for chromospheres there. One such indicator would be the very strong resonance doublet of Mg II, which shows such strong emission probably just because of the greater abundance of magnesium. You get some idea of their greater strength just by comparing them in solar spectra with the weak solar K line. The emission cores of the Mg II lines are enormous by comparison. So that's one additional chromospheric indicator, which has a certain disadvantage in that it must be observed from above the earth's atmosphere. We should also search for other indicators, and among those, I have suggested that emission lines might be very valuable. In order to determine whether an emission line is intrinsic, and so a good indicator of a chromosphere, we have first to solve the problem of what an emission line means. In particular, is it intrinsic or geometric in origin? Does this offer a partial answer to your question?

Kippenhahn — I think so. Would you suggest, then, that when we move up the main sequence, we better get observations of the Mg II lines from a balloon in order to check for chromospheres.

Underhill — Don't forget satellite observations here.

Jefferies — Yes, and if the magnesium doesn't show us an effect similar to the calcium, then I think that we've run out of chromospheres.

Underhill — You have run out of magnesium emission after the middle B's.

Thomas — Of course it's all a question of how we define chromospheres too.

Heap — Hasn't Kippenhahn's question already been answered by some of the observations discussed here earlier? For example, Kondo's observations of Mg II emission, suggests that chromospheres may be found in stars having spectral types much earlier than F4.

Kondo — I just want to mention that our balloon program was initiated in the philosophy, similar to that articulated by John Jefferies, of searching for evidence of chromospheres and of enhancing our understanding of chromospheres through investigation of the magnesium resonance doublet. I also want to add that, in future flights, we hope to address ourselves to the point raised by Anne Underhill regarding where in spectral type the magnesium emission is unobservable.

SUMMARY

O. C. Wilson

Hale Observatories

Carnegie Institution of Washington, California Institute of Technology

I was asked to summarize this Conference. However I think that I can be more effective if I stay with those matters where I have some personal experience. Accordingly, I propose to restrict my talk to what I shall call solar-type chromospheres. I hope to review some of the things that are known, to point out what seem to me to be unsolved, or incompletely solved, problems, to comment on some issues raised during the meetings, and to bring you up-to-date on some of the current investigations. In this way, I hope to make some points of interest to the observers as well as to the theoreticians.

By solar-type chromospheres I mean two things: First that the H-K reversals satisfy the well-known width-luminosity relation; second that the morphology of the reversals is essentially of the common double peaked form which is familiar from the Sun. The cross-hatched region in the schematic H-R diagram shows where such chromospheres are found; on the main sequence from F5 down, and in the giants from G0 to later types.

It is important to realize that a chromosphere is a completely negligible part of a star. Neither its mass nor its own radiation makes a significant contribution to those quantities for the star as a whole. Moreover, I know of no essential role that a chromosphere fills in the life of a star. For example, there are places on the main sequence where stars can be found which must have identical masses and energy productions, but whose chromospheres are very powerful in some, and absent, or almost completely so, in others. The stars in question function equally well with or without chromospheres. Hence an outsider might be pardoned for asking why this many people have spent four days here studying something which seems as nonessential and insignificant as chromospheres.

Actually, of course, the motivations for people to engage in this particular type of research are as varied as their own interests and specialties. In my own case, I have been attracted to chromospheres in the first place by pure curiosity and secondly by the fact that they turn out to be packed with information. By proper study of stellar chromospheres and, indeed, thus far involving only the H-K reversals, it is possible to derive very valuable knowledge about the absolute luminosities of late-type stars, about the ages of stars on the main sequence, and, more recently, about the occurrence of stellar analogs of the solar cycle. I have hoped that the

theoreticians would be sufficiently intrigued by all this to provide believable explanations for the physical processes which underlie this wealth of information.

I shall return to some of the foregoing matters later. But first let us look at the schematic H-R diagram in Figure IV-11 and consider the boundaries contained therein. These are two in number: first, the one on the

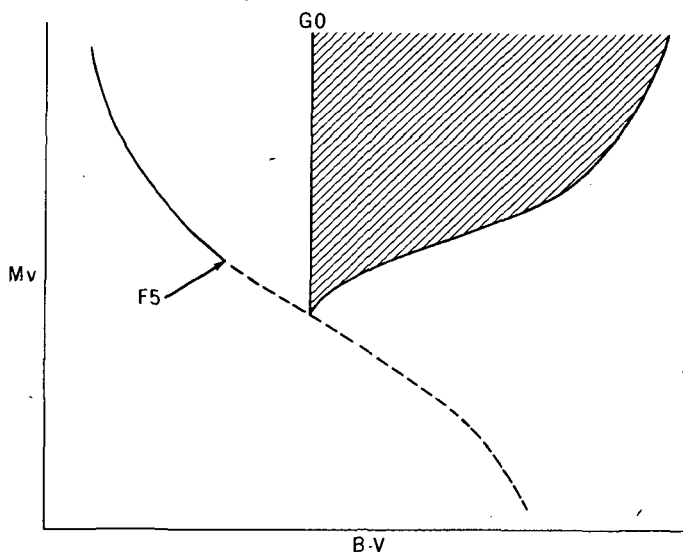


Figure IV-11 Schematic HR Diagram. Cross hatching shows regions of occurrence of solar-type chromospheres.

main sequence at spectral type approximately F5, and the other, which probably is essentially a vertical line through the giant and supergiant region, corresponding closely to spectral type G0. In the present context these boundaries separate those regions in which H-K emission can be seen readily at a dispersion of 10 \AA mm^{-1} from those in which H-K emission is invisible at this scale. Boundaries of this sort are likely to mark a place where some important physical change takes place and are therefore worthy of intensive study.

The main sequence boundary has been investigated much more thoroughly than the one in the giant region. The result is that the point on the main sequence where strong chromospheric emission terminates coincides, with great precision, with the point where the larger rotational velocities cease, as one proceeds down the main sequence from earlier spectral types. In fact, the mass range within which these two transitions occur can be only a very few percent at most. Since the deep hydrogen

convection may also set in at approximately this same point, according to theoretical studies, it seems to me likely that both transitions, from weak to strong chromospheric activity, and from large to small rotational velocities, are due to the onset of deep convection. The one remaining necessary link in this argument, that braking of rotational velocity is due to ejection of charged particles by chromospheric activity, and interaction of these particles with the magnetic field lines of the rotating star, has been supplied by Schatzman some years ago. Of course, even though this picture of what occurs at the transition zone on the main sequence appears to be both reasonable and consistent, it may not be correct, and one must be prepared to consider other interpretations.

The other boundary, in the giant region, requires further study. I have taken a few spectrograms of luminous stars of types F7-F9 and I have the distinct impression that, for these stars, the H and K lines become suddenly very much deeper than at G0, and the only emission, if present at all, appears merely as slight shoulders well down in the lines. This boundary should be investigated more completely than it has been to find the real nature of the chromospheric transition.

I should like now to comment on some recent developments, mostly unpublished as yet. The width-luminosity relationship was derived originally by making use of the MK standards of appropriate spectral types and the line widths were determined in the simplest fashion by setting the cross hair of an ordinary measuring engine first on one edge and then on the other edge of the emission lines. The result is a linear correlation between $\log W_0$ (W_0 is the width in km s^{-1} after correction for instrumental width by subtraction of a constant) and the absolute visual magnitude, M_v ; and this extends from stars of absolute magnitude -5 or more down to the faintest stars on the M.S. for which the dispersion of 10 Å mm^{-1} is adequate, i.e., to +6 or +7. A recalibration using the Sun (high dispersion solar spectrograms in integrated light) and the yellow giants of the Hyades agreed very closely with the original one based on the MK standards. There is an admitted weakness in this calibration for the more luminous stars, since only ζ Aurigae and some of the bright M-type supergiants in η and χ Persei could be used as checks, although they too showed good agreement.

There have been various criticisms of this method of deriving absolute magnitudes, the most serious ones raising the question of a dependence upon the abundance ratio $[\text{Fe}/\text{H}]$. I have recently been trying to shed some light on this matter, in collaboration with two colleagues at the Copenhagen Observatory, by observing certain physical pairs of stars. In these pairs, the primary is a G-K type giant and the secondary is a main

sequence star of type A-F. Spectrograms of the primary yield its absolute magnitude on the basis of the Sun-Hyades calibration. The Copenhagen observers use the *uvby* system of Strömgren to derive the absolute magnitude of the secondary and the apparent magnitudes of both stars. Their photometry also yields values of $[\text{Fe}/\text{H}]$ for both stars and the average difference in this quantity between the members of 18 of the pairs is only 0.09, which is very good agreement. Reduction of incomplete data for these 18 pairs shows that the Sun-Hyades calibration agrees with the Strömgren absolute magnitudes to an average difference of only 0.1 mag. The range of $[\text{Fe}/\text{H}]$ encompassed is about 0.7, and over this range there is no definite evidence of dependence upon $[\text{Fe}/\text{H}]$, but final completion of the project must be awaited before drawing more definite conclusions.

I refer now, briefly, to the question of ages of stars on the main sequence. All the evidence, and there is by now an impressive amount, indicates that the degree of chromospheric activity, as measured by the strength of the Ca II emission in a main sequence star, is indeed a decreasing function of its age. Thus, at the present time, it is quite possible to observe, say, all the K_0 stars in the solar neighborhood in the proper way, and put them in the right order of age. This is, in itself, a valuable tool. But even more important is the work being done by Skumanich to calibrate the rate of chromospheric decay in absolute terms. I think it is possible now to look forward to the time when the actual ages of all main sequence stars from F5 down, within reach of the appropriate equipment, can be specified in years. The value of such data in, for example, the study of galactic stellar orbits as a function of age is obvious.

Calcium spectroheliograms have made it evident for a long time that the radiation in the chromospheric Ca II lines in the sunspot zones waxes and wanes in synchronism with the other indices of the solar cycle. If the stars behave in similar fashion, then, by monitoring the emission in these lines against the adjacent continuum, one should in principle be able to find and study stellar analogs of the solar cycle, and to determine the shapes, amplitudes, and periods of any cycles which occur. Since all theories of the solar cycle have, of necessity, been restricted to reproducing the features of the solar cycle itself, it may well be that they lack sufficient generality, and an extension to other stars should greatly improve this situation. To make this kind of observation it is essential to be able to isolate accurately narrow bands at the centers of the stellar H-K lines and to measure the flux in these bands with respect to the nearby continuum with precision.

The coude scanner at the 100-inch telescope is an ideal instrument for this type of work, and, since 1966, I have been measuring the H-K fluxes in a number of main sequence stars from spectral type F5 to M_0 . It is not feasible here to go into either the instrumental details or the results thus far obtained. Very briefly, it turns out that the earlier type main sequence stars, F5 to G5, have not so far shown variations that appear to be cyclical. Either they do not have them or their periods are too long compared to the time of observation. However, beginning at about type G8 there are perhaps a dozen stars whose variations could very well be cyclical in nature, though none have yet been followed through a complete period. Noteworthy also is the fact that not all stars of the same types show the same kind of behavior. A few more years of observation should settle some of these questions definitely.

One item which has not been mentioned in this Conference, but which I think may well be of importance and which deserves further study, is the occurrence of He in emission in a number of stellar spectra. I first noticed this line in the spectrum of Arcturus in 1938, on a high dispersion plate. In this example, the He line has about the same width as the H-K emissions, but unlike them it has a smooth rounded top with no evidence for a central dip. It has seemed to me that these facts must contain important clues to chromospheric conditions, especially, perhaps, the approximate equivalence of line width for a ratio of atomic weights of 40:1.

Another topic which has been mentioned several times at this meeting is the well-known enhancement of chromospheric activity in the members of close binaries, a point which has been noted in the literature by several individuals. Here again is a field in which more systematic observation might succeed in shedding light on chromospheric mechanisms. I wish merely to call attention to what I consider a rather spectacular case which I came upon while looking for Li lines in binaries, and which I believe might repay further study. This is the bright member of the visual pair ADS 2644, and I published a brief note about it in *P.A.S.P.* 1964. This star is a spectroscopic binary, of spectral type G9 V. The H-K emissions are very strong and much wider, about 1 Å, than is normal for a star of this type and luminosity. But at H α the situation is even more abnormal; here the usual H α absorption line is completely masked by a strong, broad emission band of width 5 to 10 Å. Evidently the presence of the companion has induced very large velocities in the chromosphere of this star, and the velocity spread in the region of H α formation appears to be several times larger than that where the Ca II lines are formed. It is not impossible that further study of this and similar systems might yield information useful in the understanding of normal, undisturbed chromospheres. In any case, the relationship of hydrogen and calcium line widths

in this star is strikingly different than their relative widths in the presumably undisturbed chromosphere of Arcturus.

There has been mention of surface magnetic fields during this meeting, and of their role in chromospheric excitation. I think there is general agreement on the part of theoreticians that such fields are necessary in the transfer of mechanical energy from the hydrogen convection zone and in its deposition in the chromosphere and corona. However, there seems to be some disagreement as to the source of the fields. If we appeal to the observations, we have seen that on the M.S., below the boundary at spectral type F5, a star begins its main sequence life with strong chromospheric activity, but this activity gradually diminishes and may, in time, cease altogether. This can hardly be due to any change in the hydrogen convection zone whose existence depends only on the general parameters of the star, the great abundance of hydrogen, and the latter's high ionization potential. I cannot see what remains to explain the decrease in chromospheric activity except to suppose that the surface magnetic fields decrease with time, presumably because the magnetic energy is used up by transformation to energy of other kinds. This could happen if the magnetic energy in question is a residual left over from the star's extreme youth and not replenishable. But if it is produced by a dynamo within the star, then the dynamo must run down and effectively cease to operate. It is up to the theoreticians to decide which, if either, of these two views is the more acceptable.

There is, however, one more clue to be obtained from the observations. We see that when stars which have been sitting on the M.S. for a time long enough to reduce their chromospheric activities to very low values begin to evolve up the lower boundary of the giant region in the H-R plane, they need to go only a little way before their chromospheres reappear. Is this because some internal magnetic field has been allowed now to reach the surface, or has the internal dynamo been reactivated? I do not pretend to know the answer, but I feel that there are some fundamental and fascinating questions awaiting investigation.

The study of stellar chromospheres, either for themselves or to abstract the information they contain, is essentially a question of high, or at least medium, dispersion spectroscopy. I wish to call the attention of the observers to some recent instrumental developments which I believe portend gains in this field fully equivalent to those which resulted from the introduction of photography into astronomy a century ago. These developments, insofar as I am aware of them, are two in number. The first involves a silicon diode device which has not yet been applied to spectroscopy, but which appears very promising. The other has already produced very spectacular spectroscopic results. I refer you to a recent

paper in the *Astrophysical Journal* (J. L. Lowrance *et al.*, 171, 233, 1972). These authors succeeded in obtaining, in six hours, a spectrogram of dispersion of 9 \AA mm^{-1} of a QSO of magnitude 16.5.

The implications of this work for stellar spectroscopy are very impressive, especially since it is reasonable to anticipate improvements in the apparatus in the course of time. Let us consider only the application of the width-luminosity relationship to the determination of absolute magnitude as an example. It thus appears probable that in the relatively near future this method will become applicable to the red giants in a number of globular clusters, to the similar stars in many open clusters, and to vast numbers of non-cluster stars of special interest. Moreover, for stars which are now very difficult to handle at 10 \AA mm^{-1} , higher dispersion and increased accuracy should become easy. I cannot help feeling that coude stellar spectroscopy, including of course the study of chromospheres, is on the verge of a new era of accomplishment and vastly increased capability.

Finally I wish to address myself to the theoreticians. They have done much work, involving what might be called the standard theory, in attempting to explain the chromospheric emission lines. Their tools are non-L.T.E. theory, the equation of transfer, and source functions. I must confess that I have some misgivings about the applicability of their results to the real world. As an example, and I have seen others at various meetings, I refer to Dr. Avrett's worthy efforts to account for the width-luminosity relationship. By scaling up the quantities applicable to the Sun, he derives emission lines for a giant star, and they are indeed wider than the solar lines. But, unfortunately, they are quite different in shape from the lines one sees in nature. They have rather extensive wings, whereas the real lines have very well-defined edges which must be very steep. Indeed, as a rough first approximation, the real lines must have edges which are nearly vertical; if this were not so the simple measurements which are employed in applying the width-luminosity relationship would not work, as they do, whether the lines are weak or strong. Incidentally, Avrett wishes to attribute the width-luminosity relationship to the effect of surface gravity, g and this carries the implication that p must be constant across horizontal lines in the H-R diagram, which is not the case.

I have the feeling therefore that the theoretical explanation of the chromospheric lines is presently incomplete. Perhaps some of the parameters involved can be modified so as to arrive at good agreement and still remain within the believable range. But I suspect that an essential ingredient may have been left out and that this ingredient may be the velocity distribution of the radiating elements. The problem may be one

of hydrodynamics as well as of transfer theory. In any case further work is urgently required. First, high dispersion stellar spectrograms should be processed with care in order to define accurately and quantitatively what the properties of the chromospheric lines really are. Then the theoreticians will have to reproduce these lines as best they can, even if it requires the introduction of additional parameters.

I have tried to give here a brief but fairly complete view of the current status of the study of stellar chromospheres. We have learned a few things, but I think the subject is still in its very early stages and is deserving of much more effort on the part of observers and of theoreticians. To me, one of its most attractive features is the curiously large number of contacts with other astronomical fields to which it is able to make contributions.

CONCLUDING REMARKS FOLLOWING THE SUMMARY

Thomas — Dr. Wilson was asked to summarize the conference, as it is customary to have someone with wide experience and breadth of knowledge in the field close such a symposium as this on a note of perspective. It is not necessary that he be an expert on all the matters covered; one hopes only to hear some sort of encompassing “impressions” of what we, the participants, have been exposed to, and how well it “registered” to one having a broad background. I, personally, regret that Dr. Wilson chose not to do this, because I think that we would all have benefited greatly to hear his impressions. But I think that someone should try to do it, both for the sake of those who have tried to present a digest of ideas and for those of us who have just listened and commented. Otherwise, one may be left with what I consider the mistaken impression that there is only one type of chromosphere really worth much attention, the solar type, and only one set of indicators of the universality of the chromosphere phenomenon, those relating to the H and K lines. So, let me attempt a rather general summary.

First, I can say in an overall way that I disagree strongly with Dr. Wilson on his assessment of the general importance of chromospheres. If I follow the logic of Dr. Praderie, in her presentation, that the properties of a stellar atmosphere may be discussed in terms of two kinds of fluxes — electromagnetic radiation and mass — then *conceptually* the chromosphere is that part of the atmosphere directly dependent upon a non-zero mass flux generating a mechanical energy flux.

Also, in a wholly *observational* way, the chromosphere determines the properties of the cores of most strong lines in the solar (and most of the stellar) Fraunhofer spectrum: not what I would call an irrelevant thing.

Indeed, I well remember a discussion in the 1950's as to whether the solar chromosphere had *any* observational consequences on the Fraunhofer spectrum. And it was a major milestone in solar research when it was shown, unambiguously, from eclipse studies, *just how many* solar lines observed on the disk were influenced by the properties of the chromosphere. As an indicator of the existence of a mass-flux, and as a determiner of the properties of the cores of both strong and intermediate lines — I hardly consider the chromosphere as a “negligible” part of the structure of a star. If I venture to comment on the direction from which K. Gebbie, Pecker, Praderie, and I have been working — which has evolved into viewing the atmosphere as a transition region between stellar interior and interstellar medium — the chromosphere is again a most important region in this transition, from the direction Dr. Praderie emphasized. So, having tried to restore the role of the chromosphere into focus, let me try to survey what the invited speakers summarized for us.

Beginning at Day 1, which, in essence, was theory, Jefferies made two major points:

- How can one find the temperature structure of the chromosphere? He noted that there are two kinds of lines: ones which have collision-dominated source-sink terms, like the Ca^+ and Mg^+ H and K lines, and ones which have photoionization dominated source-sink terms like the Balmer series of hydrogen. In the case of the former, you can tell something of the T_e structure; this is true particularly in the case of an atmosphere with a temperature reversal. Such a reversal may produce a central emission core, and the central emission core may be, in turn, reversed wholly by radiative transfer effects. This is in contrast with the old L.T.E. interpretation which required a second temperature reversal to produce the self-absorbed core. In the literature, there are a lot of predictions of these kinds of effects, ranging from lines with no self-reversal to lines with self-reversals. You can make the self-reversals as strong as you want to, as wide as you want to, and the emission core as steep as you want to by “choosing” arbitrary distributions of T_e . For example, see Lemaire's thesis on the Mg II H and K lines. Now Wilson is interested in square profiles, i.e., profiles with steep sides to the emission peaks. Strictly speaking, there is no such thing as a “square profile”; it is “square” only to some accuracy. Very steep sides on profiles have been computed, however, for particular atmospheric configurations, and they are in the literature. Furthermore such variations in steepness and behavior of the central core are found observationally in the Sun. Again, refer to the Lemaire thesis as an excellent compendium of observation and theory.

- Jefferies second point concerned the observed emission lines and how their existence may relate to the existence of a chromosphere, emphasizing the distinction between intrinsic emission lines and geometrical emission lines. If we consider spectral regions where the continuum is depressed, we can have either kind of emission line. In the visual regions, where the continuum is not depressed, we obtain emission cores in absorption lines as a reflection of an intrinsic emission line. We can have any combination of these, depending on circumstances. The following approach by Avrett permits a demonstration of these points.

The summary by Avrett showed what one could and could not do with various models, i.e., various *assumed* distributions of T_e . It was numerical experimentation. Its approach is one that Wilson could call upon to ask, "Can I, under any circumstances, get theoretically such-and-such a profile," and "How many kinds of circumstances can produce it?"

Now, as a comment on the bearing of these H and K profiles on our ideas about chromospheres, and as a bridge to Dr. Praderie's summary, let me quickly summarize the evolution of the past 25 years in our outlook.

In phase 1, the only star which had a chromosphere was the Sun. And the textbooks of that time (1950's) said that the chromosphere had absolutely no influence on the observed disk spectrum of the Sun. There were observations of line profiles which apparently showed (under LTE diagnostics) that the limb temperature was as low as 2700° K, in conformity with the LTE line blanketing calculations. That was the end of phase 1, essentially wiped out by the body of non-LTE theory applied to interpreting solar eclipse observations, which among other things showed such temperatures to be erroneous.

In phase 2, which Wilson's talk summarized masterfully, there were admitted other stars, besides the Sun, with chromospheres, and it was thought that these were essentially measured by H and K self-reversed emission cores. Recognition of these other chromospheres was an enormous step forward. Such stars occupy some part of the HR diagram, and about this part we have considerable "suggestive" information coming from those empirical relations which Wilson discussed. These tell us that there is some profound relation between the energy production by the star and that fraction of it which goes toward providing a chromosphere.

In phase 3, we advance to the rest of the HR diagram, so long as one makes synonymous the concept of a stellar chromosphere and the existence of Ca⁺H and K line. No stars here were supposed to have chromospheres; cf. the 1955 IAU Symposium and comments by Biermann and Schwarzschild.

In phase 4 we admit a chromosphere may exist in stars which do not have H and K as the major chromospheric indicators; and we begin an open-minded search for what these other indicators are. So, we broaden our sights, and we are here at this conference.

On Day 2, Dr. Praderie emphasized two conceptual points. First, a *necessary* condition for a chromosphere is a mass flux, taken in the broad sense of mass motion somewhere in the star. Second, a *sufficient* condition for a chromosphere is mechanical dissipation. She then described the direct observational evidences for chromospheres, only one kind of which is provided by the H and K lines. These H and K lines stand out in the minds of all of us because the lines are so well observed and because there is some kind of theory to interpret them. As you go to more complex atoms, there are complications, i.e., multilevel atoms, etc. One cannot predict theoretically all the features Dr. Praderie talked about, but she divided them into two aspects: excitation phenomena and ionization phenomena. For example, just the existence of helium lines on the solar disk and in the solar chromosphere tells us right away that there is some kind of anomaly. These are all direct observations. Praderie then went on to the indirect evidences for chromospheres — the existence of velocity fields of one form or another. This aspect might have been discussed by John Jefferies in his review of diagnostic techniques, but one must have a great deal of sympathy for why he did not cover these things, since our explanations and our analysis of the existence of velocity fields are extremely rudimentary so far. We take the direct diagnostics as giving some evidence for a temperature rise, and the indirect diagnostics as giving some evidence for the possibility of mechanical dissipation, which may then produce a temperature rise. While Wilson stated that there is general agreement among theoreticians on the necessity for magnetic fields for the transfer of this mechanical energy, I think this is a misleading statement. All the original work on chromospheric heating by mechanical dissipation ignored magnetic fields. One currently invokes magnetic fields to understand differential heating over the solar surface. I believe the question of the relation between mass flux and mechanical dissipation and magnetic fields is most important but badly understood at present. While simple correlations between the presence of magnetic fields and Ca^+ emission are excellent guides, a theoretician cannot afford to depend wholly on them. Agreed, one needs empirical relations to start and to be stimulated, but one needs to go *far* beyond that. Also, this coupling between the velocity fields and the H and K lines is a very strong point right now. The problem of interpreting the half-widths of these lines, and the H α lines, and all the other lines Dr. Praderie discussed, is a very real one. It all comes down to indirect indications of chromospheres: the indications of potential chromospheric heating in the presence of velocity fields.

Doherty's summary put very well those aspects which have been exciting to all of us who had to live so long on the observations in the visual spectrum; viz, the enhancement in the "space" ultraviolet of all these things that one could only guess at from the cores of the H and K lines. The balloon observations of the enhanced Mg^+ emission cores provide a direct extension of the Ca^+ material. Then, we have in great profusion P Cygni-like lines showing evidence of outflow of mass, which links strongly to the theoretical work by Parker and subsequent work on the solar wind. When we find evidence for many lines showing P Cygni characteristics, plus many emission lines in stars which cannot be interpreted wholly in terms of geometrical effects, then we have enormously powerful chromospheric indicators.

I think that if the theoreticians are to be criticized, it should be in a tough but realistic way. And the tough way is that the theoreticians have not provided simple, straightforward models, both of the physical concepts underlying all this non-LTE diagnostics and of the physical concepts underlying mechanical heating and really non-equilibrium thermodynamics, in such a way that the observer can both see it clearly, and can sit down and make simple-minded approximations in order to interpret these space observations. Non-LTE theory is not conceptually that complex. That is my summary of the first two days.

In some sense, the third day was the real meat of the conference to those of us who are concerned with the definition of a chromosphere in terms of mechanical heating. The preponderance of thinking in this symposium has been to define a chromosphere in terms of a T_e -rise, because we know what that predicts.

This is the real focus of the conference so far as many of us are concerned. But, we are staggering. We have some kind of diagnostics developed; we have enormous numbers of observations; we have from Wilson and his co-workers enormous stimulation so far as one kind of chromosphere is concerned, that kind centered on the H and K lines as a diagnostic tool, suggesting, in the Wilson-Bappu relationship, that there is some correlation between the intrinsic luminosity of the star and that part of the mass flux which provides a mechanical energy dissipation to heat the chromosphere. How do we explain it? If you go back to the early days, when these very first suggestions on mechanical heating were made by Biermann, Schatzmann, and Schwarzschild, then we have very naive ideas, to which reference has been made today. One goes on from there to ask: how do I produce, first, the flux from a given internal convective structure; how much flux do I produce; how do I get up into the regions of heating; and where do I heat the atmosphere? There were strongly technical discussions on day 3, and certainly, those presenting

the discussions did not bring us all up to their level. But there were two interesting summaries: One was by Jordan who summarized the applicability of various approximations on when a sound wave becomes not just a sound wave but something strong enough to produce heating in the atmosphere. That is the sort of investigation we need to explain the Wilson-Bappu relationship. Jordan summarized the current thinking on that kind of approach. The emphasis lay on the basic physics. The second summary by Delache was an attempt to go back from that standpoint and to ask, what do I do when I talk about those phenomena which produce a chromosphere or a corona? And you start from the very basic thesis by Parker that you can't have quiescent stars, so long as you do not have a constraining boundary in some sense. He went on from there to develop what possible kinds of structures one could have, recognizing that the Parker stellar wind means that all the way down into the star some kind of a mass flux must exist, no matter how small in the deepest layers. This is the kind of approach one needs to begin to make some kind of theoretical structure. If I only try to say that all I have is a variety of motions of unknown origin in the solar atmosphere, and it is their resultant that produces the observations, introduced in an ad hoc way, I go to a situation similar to terrestrial meteorology. It is like saying there is no point in making a first-approximation model of the terrestrial atmosphere because I can not reproduce *all* the local phenomena that you see when looking out the window of an airplane — lightning discharges, beautiful clouds with periodic structure, enormous plumes, etc. The answer to that viewpoint is that it is simply defeatist. One has to do the best he can to start. What do we do? First we make a spherically symmetrical model of the stellar interior, and then a spherically symmetrical model of the stellar atmosphere, not because we believe that is the last word; but each time we made a model, we should say, "That model is good to some degree of accuracy." We make models to be compatible with the observations, good enough to achieve internal physical consistency; and then we try to reproduce our observations. All Day 3 was trying to tell us was the accuracy to which we know the basic physics; namely, how much mechanical flux is put in the atmosphere, how much is stored, how much is propagated, how much is dissipated to the accuracy that we know initial boundary conditions; all in the hope that, with this knowledge, we can use those results on two things — mechanical dissipation of energy and velocity fields.

On Day 4, Kippenhahn gave what I consider to be a fine complimentary summary of the work that Wilson has presented here. Kippenhahn gave essentially a theory behind this particular kind of chromosphere, based on the internal structure of particular stars. He presented for us a very beautiful, complex, "flow diagram" of the linkage paths between mass

loss, angular momentum loss, magnetic field from the turbulent dynamo and its relation to differential rotation and the convection zone, and stellar evolution. Somehow, he suggested these are measured by g and T_{eff} — myself, I have a hard time seeing how these two parameters suffice — but this probably just reflects my own ignorance, which is a good admission for a summarizer to make.

That is what we have had in the conference: some diagnostic techniques; a summary of observations of different kinds of chromospheres that appear to exist; a summary of the theory for some very particular effects, namely the aero-dynamics as we know it today; and a summary of the observations of some particular stars, following a summary of the relation of the interior structure of certain types of stars to chromospheres.

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